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AN ATTEMPT TO TEST THE NEBULAR HYPOTHESIS BY AN APPEAL TO THE LAWS OF DYNAMICS.

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1. *Introduction.* — One of the boldest and most attractive speculations ever offered in any science is beyond a doubt the hypothesis of the development of the solar system from a vastly extended nebula, which was formulated by Laplace a little more than a century ago. His unparalleled researches on the mechanical causes of the forms and intricate movements of the planets and satellites, and his unprecedented success in employing the law of gravitation as an instrument of investigation, gave him a deep faith in the sufficiency of Newton's law for the explanation of all the facts relating to the present constitution and past development of the solar system. He was struck by the really remarkable circumstance that all of the bodies of the system, both planets and satellites, so far as had then been determined, moved in nearly the same plane, and revolved and rotated in the same direction. He came forward with an array of figures derived from the Theory of Probability showing that such a

number of practical coincidences in so few chances could happen, at hazard, but once in countless millions of times. He concluded that the present arrangement is not fortuitous, but that it has had its cause in the original dynamical relations of the system and the laws that have been operating upon it. It must be remarked, however, that he propounded his hypothesis with "that distrust which everything ought to inspire that is not the result of observation or calculation."

Perhaps no other theory in the natural sciences, which has not been rigorously demonstrated to be true, has found so speedy and unanimous acceptance, or has stood so long without radical modifications. This is all the more noteworthy since it was the first explicit formulation of the theory of evolution, a theory which has been so hotly contested in other fields of thought. It has exercised a marked influence on most of the speculations in geology and astronomy until the present day. It has been accepted with a few slight modifications almost without question by the highest authorities, as Helmholtz, Kelvin, Newcomb, and Darwin, and it has been made the basis for the most sweeping conclusions. It has held such a dominant sway over the thoughts of investigators in the field of the grosser modes of inorganic evolution that doubtless in many instances facts have been warped and perverted, and questionable methods of reasoning employed, in order to bring experience into harmony with it. But this is an age characterized by a critical attitude toward fundamental assumptions in all fields.

In this paper it is proposed to question the exactness of the Laplacian hypothesis, not only in its original form, but also as modified by the discovery of new members of the solar system, and by the establishment of the law of the conservation of energy. In particular, the inquiry will be raised whether it is compatible with the fundamental laws of dynamics.

The methods of testing the theory will be divided into three categories: (I) comparison of observed phenomena with those which result from the expressed or implied conditions maintained by the hypothesis; (II) answers to the question whether the

supposed initial conditions could have developed into the existing system; (III) comparison of those properties of the supposed initial system with the one now existing, which are invariant under all changes resulting from the action of internal forces.

The first category will contain the objections which have been usually cited as contradicting the theory, and some new ones of the same type. The second category will deal with problems which are of the very greatest difficulty when we attempt to restrict ourselves to rigorous reasoning, and the results obtained are not all that could be desired; yet it is believed that they are sufficient to be of considerable interest. The troubles arise in attempting to prove that certain things cannot take place in any time, however long. Mathematically stated, it is difficult to find solutions of the differential equations involved which are uniformly convergent for all values of the time. The third category will make a comparison of those properties which must remain constant. They are defined by *integrals* instead of by solutions, as in the former case, and have the important property of being independent of the time. They are such as the mass of the system, the invariability of the plane of maximum areas, and the constancy of the moment of momentum.

In carrying out the numerical work involved some approximations have been made for the sake of simplicity, but it will be noticed that in every instance they have been chosen in such a manner that they favor the validity of the nebular theory. It follows, therefore, that the adverse conclusions arrived at are just as certain, so far as their bearing on the theory is concerned, as though numerical exactness had been insisted upon throughout.

The results contained in this paper have been arrived at jointly by Professor T. C. Chamberlin and myself as the result of a more or less continuous study of the subject during the last three years. In this time we have had many conversations in regard to this field of science where geology and astronomy meet. Our exchanges of ideas have been so frequent and so complete that it is not possible to divide the responsibility for

the various methods of attack, and the manner of carrying them out. Yet, as a general rule, it may be said that Professor Chamberlin has employed his keen perception of physical relations, and his exceptional powers of invention, in the formulation of problems relating to the general theory, while it has been my office to investigate their pertinency and bearing by appeals to the mathematical principles of dynamics. This order of procedure has not been universally followed; indeed, in some instances the results have been reached by successive steps made alternately by one and the other of us. In a paper in the *Journal of Geology* published nearly concurrently with this, by mutual understanding, Professor Chamberlin outlines some of the studies which have led up to the present inquiry and indicates their geological relations.

2. *Definition of the nebular hypothesis as treated.*— In order to remove all uncertainty in regard to the precise hypothesis which is under discussion, it will now be briefly outlined. This is the more needful because it will be considered in its broad outlines so as to avoid urging objections of a special nature founded on particular assumptions regarding temperatures and physical conditions of matter.

The nebular theory examined in this paper supposes that at some remote epoch the materials of which the solar system is composed were in a gaseous or meteoroidal state, filling a space approximately spheroidal in form, and extending at least to *Neptune's* orbit. If the gaseous condition is assumed, it does not insist upon any particular temperature, but merely supposes that the whole mass was in hydrodynamical equilibrium, rotating practically as a solid with an angular velocity equal to that of the outermost planet. If the meteoroidal condition is assumed, it supposes that the properties of gases were nearly fulfilled in accordance with the conclusions reached by Darwin in his memoir, "On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony," which he presented to the Philosophical Society November 15, 1888. Therefore the system may henceforth be spoken of as being a nebula. The

theory supposes that the nebula has been subject to no appreciable external forces, and that it has contracted under its own gravitation, either (*a*) leaving off rings at certain intervals, or (*b*) dividing by some fission process, and that these separated portions have contracted, forming the planets and the satellite systems. The comprehensive theory under discussion is thus seen to include a considerably wider range of possible initial conditions and modes of development than that postulated by Laplace. There is no limiting assumption here regarding the temperature or the original physical condition of the matter or the manner of separation. A few of the tests brought forward will bear especially upon particular assumptions, but for the most part they are general in their application.

I. METHODS OF THE FIRST CATEGORY.

3. *On the planes of the planetary and satellite orbits.* — There are certain objections to the nebular theory which at once appeal to the most superficial and careless critic. The most conspicuous and best known of these is that the revolutions of the satellites of *Uranus* and *Neptune* are in a retrograde direction. The retrograde revolutions in themselves would not be so significant (since the precise process of contraction into the planets is not described) if it were not for the additional fact that the planes of the orbits of the four satellites of *Uranus* are almost perpendicular to the plane of the planet's orbit around the Sun, while the orbit of the satellite of *Neptune* has an inclination of nearly 35°. One of the requirements of the theory seems to be that the planes of the planets' equators and of their satellite systems shall not differ much from the planes of their respective orbits. It was shown by Darwin in his memoir, "On the Secular Changes in the Elements of the Orbits of a Satellite Revolving about a Distorted Planet," which he presented to the Royal Society in 1880, that tidal friction would under certain circumstances increase the mutual inclinations of the planes of the orbits of a satellite and its planet. There is no suggestion, however, that it could change the inclinations more than a few degrees, and

Darwin says in the same memoir, on page 886: "The retrograde motion and high inclinations of the satellites of *Uranus* are, if thoroughly established, very remarkable." These facts point to a condition of heterogeneity quite incompatible with the distribution of density in a system in hydrodynamical equilibrium.

Another thing of the same nature which could be reasonably expected, under the hypothesis, is that the planes of the planets' orbits would become more nearly coincident as the Sun is approached; for the nebula would tend to rotate more nearly as a solid as the time increased and the secular results of friction accumulated. The observed facts are quite the opposite of this, the plane of *Mercury's* orbit presenting a greater deviation from the average of the whole system than that of any other planet.

Likewise it could be reasonably expected that the orbits of the planets near the Sun would be more nearly circular than the orbits of remote planets, but quite the reverse is true.

4. *On the distribution of mass in the solar system.*—The masses of the various planets are not what one would expect if the ring theory is true. If the masses of the several planets are divided by the radii of their respective orbits, numbers are obtained which are proportional to the products of the densities and the cross sections of the respective rings. Choosing the units so that the result shall equal unity for the Earth's orbit, we have:

Planet	Product of cross section and density	Planet	Product of cross section and density
Mercury	0.123	*Jupiter*	61.064
Venus	0.592	*Saturn*	9.938
Earth	1.000	*Uranus*	0.761
Mars	0.070	*Neptune*	0.566

If it be supposed that the ring from which any planet has been formed extended half way to the two adjacent orbits, then the numbers above, divided by the squares of those representing these respective distances, will give the numbers proportional to the mean densities of the various rings. Choosing the units so that the result shall equal unity for the Earth's orbit, we have:

Planet	Density of ring	Planet	Density of ring
Mercury	0.150	*Jupiter*	0.609
Venus	1.010	*Saturn*	0.028
Earth	1.000	*Uranus*	0.0012
Mars	0.003	*Neptune*	0.0008

The irregularities of these numbers are such that it seems probable that the nebula was very heterogeneous.

5. *On the inner satellite of Mars and the rings of Saturn.*—Since the discovery of the satellites of *Mars*, by Hall, in 1877, the rapid revolution of *Phobos* has been urged as an objection to the nebular hypothesis; but the difficulty has been explained by Darwin in a memoir presented to the Royal Society in 1881, as being the necessary result of tidal evolution. He says in this memoir, "On the Evolution of the Solar System," p. 534, "That the inner satellite of *Mars* revolves with a period less than a third of its planet's rotation is perhaps the most remarkable fact in the solar system. The theory of tidal evolution explains this perfectly, and we find that this will be the ultimate fate of all the satellites, because the solar tidal friction retards the planetary rotation without directly affecting the satellite's orbital motion."

The reasoning employed by Darwin is certainly correct qualitatively, and the quantitative conclusions may perhaps be accepted as sound also. They will at least be adopted here, and on the basis of this it will be shown that a more serious difficulty arises in considering the revolution of *Saturn's* inner ring, and that it cannot be explained away in the same manner.

The tides raised by the satellites of *Mars* have been so small that tidal reactions cannot have modified their orbits appreciably; and conversely, they cannot have been effective in reducing the planet's rotation. If it be assumed that *Mars* actually extended to the orbit of *Phobos*, and had a rotation equal to that of the revolution of the satellite, and that all the reduction in the moment of momentum implied by the shrinkage from this dimension has been brought about by the action of the Sun's tides, then we have a standard of comparison. In considering the magnitude of the effect of the Sun's tides a sufficiently

approximate result will be obtained by assuming that *Mars* has always been a homogeneous sphere. Carrying out the computation, it is found that the rotational moment of momentum of *Mars* was, when it extended to the orbit of *Phobos*, 24.61 times as great as at present.

It is clearly demonstrated by theory and spectroscopic observations that the rings of *Saturn* are made up of a great number of discrete masses, each pursuing practically an independent orbit. Because of the very small mass of the entire ring system compared to that of the planet, each body makes its own revolution in almost exactly the same period that it would if it were undisturbed by the others. Then by means of Kepler's harmonic law, and from the accepted dimensions of the ring, and the elements of *Titan's* orbit, we find that *the bodies constituting the inner edge of the inner ring perform their revolutions in 5.66 hours, while the period of the planet's rotation is known to be 10.23 hours.*

The dimensions of the ring cannot have been sensibly changed by tidal action, so it is fair to assume, in testing the nebular theory, that *Saturn* at one time extended to the inner ring and rotated in its period of revolution. Carrying out the computation under the supposition that *Saturn* has always been homogeneous, it is found that its angular moment of momentum has been reduced from 2.8 to 1. If the Laplacian law of interior density is assumed, it is found that the reduction has been from 1.8 to 1. This is much less than that which had to be accounted for in the case of *Mars;* but it must be remembered that *Saturn* is more than six times as far from the Sun as *Mars*, and that the retarding force of the tides varies nearly as the inverse seventh power of the distance from the disturbing body. The tidal retardation also depends upon the size and viscosity of the tidally distorted planet. In the memoir last quoted, in Table III, p. 526, Darwin gives in the column headed, "Numbers to which retardation is proportional," "*Mars*, 0.89" and "*Saturn*, 0.000020 to 0.000066." The difference is so great that the moment of momentum of *Saturn* could not have been reduced by the solar

tides unless they have been operating upon this planet between 5063 and 1534 times as long as they have been upon *Mars*.

It may be urged that *Saturn* has satellites which may have been the cause of the great reduction in the moment of momentum, but the appropriate computation shows that the total moment of momentum of *Titan*, which is the largest of all the satellites, is but 0.0061 of that of the planet's rotation. The masses of the satellites are so uncertain that the computation has not been carried further; but in Darwin's paper just quoted, on page 523, he says that the total moment of momentum of the whole satellite system is "$\frac{1}{30}$ or more" of that of the planet's rotation. From these figures it is apparent that the satellites cannot have retarded the planet greatly, even if the extreme supposition be made that they have acquired all of their momenta at the expense of the planet's rotation. Therefore the rapid revolution of the inner ring of *Saturn* must be considered as being more remarkable than "the most remarkable fact in the solar system."

II. METHODS OF THE SECOND CATEGORY.

6. *On the escape of the lighter elements.*—The first objection under this category applies particularly to the hypothesis of a gaseous nebula extending to *Neptune's* orbit. According to the laws of diffusion and convection of gases the molecules of all of the elements contained in the system would be scattered among the molecules of all the other elements, with a tendency on the part of those with small molecular weights and of great abundance, as hydrogen, to preponderate at the surface. The individual molecules would be moving on the average with high velocities, depending upon their temperature and their weights. Near the surface (which in the ordinary nebular theory is supposed to be somewhat distinctly defined) the velocities would in some cases, especially for the lighter elements, be so great and in such directions that the molecules would escape the system never to return, if present determinations may be trusted.[1] The

[1] Compare "On the Cause of the Absence of Hydrogen from the Earth's Atmosphere and of Air and Water from the Moon," by Dr. Johnstone Stoney, *Trans. Royal*

velocity requisite for permanent escape at the distance of *Neptune's* orbit is about 4.8 miles per second.

The difficult part of the question arises in attempting to determine the efficiency of this sort of waste in depleting the system of its lighter elements. For hydrogen and the lighter substances, the mean square velocities of the molecules would at least nearly equal the 4.8 miles per second for very moderate temperatures. It must be supposed then that they would escape at a very rapid rate, and that hydrogen, helium, and such light elements should be entirely absent, or at least very rarely found, instead of occurring in such great abundance.

While there may be ground for doubt as to the quantitative value of the rate of escape when applied to the whole system, the case becomes much clearer when the test is applied to individual planets, and the postulated rings from which they are supposed to have been formed under the Laplacian hypothesis. Every planet defines a velocity of permanent escape for its surface. According to the nebular hypothesis it must be assumed that the planets originally contained practically all of the elements and in approximately the same proportions in which they are now found in the Sun, the residual mass. Their atmospheres should, therefore, be alike and about equally extensive relatively. The facts are these: The velocity of escape in the case of the Moon is 1.48 miles per second; it has no atmosphere, and probably none of the satellites has an atmosphere. The velocity of the escape in the case of *Mercury* is 2.45 miles per second; it has no atmosphere, or at the most a very rare one. The velocity of escape in the case of *Mars* is 3.12 miles per second; it has a very limited atmosphere. The other planets, with much higher velocities of escape, have extensive gaseous envelopes probably composed of nitrogen, oxygen, and carbon dioxide, and perhaps lighter gases in the cases of the great planets. These facts

Dublin Society, 1892. Also "Of Atmospheres upon Planets and Satellites," by the same author, *Trans. Royal Dublin Society*, Vol. VI, part 13, October 25, 1897; also "A Group of Hypotheses Bearing on Climatic Changes," by T. C. Chamberlin, *Journal of Geology*, Vol. V, No. 7, October–November 1897.

correspond with the theory and support the doctrine of effective escape.

It is a noteworthy fact that none of the very light elements, as hydrogen, is found free in the atmosphere of the Earth. It may be objected that hydrogen is very active chemically, and that it would unite with something else to form compounds. This is undoubtedly true, but helium is not found in the atmosphere appreciably, as it should be if it did not continually escape, for it is very inactive and is constantly given forth from the interior of the Earth. Now, if such concentrated bodies as the Moon and *Mercury* are unable to hold atmospheres at present temperatures, is there any ground for supposing that the Earth-Moon ring, or the ring of *Venus*, or of *Mars*, could have held any of the atmospheric gases, or water vapor, at the temperatures necessarily assigned them by the Laplacian theory? Their power of control at any given point in the ring must have been very much less than that of the Moon. Computations given later in this paper will show that the self-gravitation of such rings would be less than the differential pull of the gravity of the Sun's mass, and hence they would offer essentially no opposition to molecular dispersion.

7. *On the rarity of the solar nebula and on the formation of rings.*—The fact that the solar nebula must have been exceedingly rare when it extended out to planetary distances seems to have been insufficiently considered by those who have speculated on the subject of the formation of the planets. Owing to its rotation, the equatorial diameter must have been somewhat greater than the polar, so the volume obtained by considering the nebula to have been a sphere extending to the outermost planet will be too great, and the average density determined by dividing the mass by this volume somewhat too small. On the other hand, the nebula must have been much denser at the center than toward the outer limits. We may, therefore, divide the mass by the volume of a sphere with radius equal to *Neptune's* distance from the center of the system and obtain a fair idea of what the density must have been in the outer regions. Owing

to the central condensation the density obtained in this way will undoubtedly be too great.

The density of a sphere of constant mass varies inversely as the cube of its radius. When the solar nebula extended to the orbit of *Neptune* it had a radius 6444 times that of the present Sun and a volume more than 267,000,000,000 times as great. The average density of the Sun, on the water standard, is about 1.4. Its average density when occupying the larger volume which we are considering would have been, therefore, $\frac{1}{191,000,000,000}$ of that of water. This would be increased by about $\frac{1}{700}$ of its value by the addition of the material which went into planets and satellites.[1]

By hypothesis, the volume was maintained partly by the molecules or meteorites impinging on each other and rebounding, and partly by the general rotatory motion of the whole mass. Suppose the mass to have contracted with the consequent acceleration of rotation until the centrifugal and centripetal forces nearly balanced. The question arises what sort of a rupture would have taken place when the rotation increased; or, in other words, in what part of the mass the centrifugal force would have first balanced the centripetal. By hypothesis the nebula rotated nearly as a solid. Even if it is supposed that it was a meteoric swarm it probably would have done so. Darwin states on page 65 of his memoir "On the Mechanical Conditions of a Swarm of Meteorites, and on the Theories of Cosmogony," which he read before the Royal Society November 15, 1888, "The investigation of section 11 also gives the coefficient of viscosity of the quasi-gas, and shows that it is so great that the meteor-swarm must, if rotating, revolve nearly without relative motion of its parts, other than the motion of agitation."

When a mass rotates as a solid the centrifugal force varies directly as the distance from the center. If the mass were spherical in form and homogeneous the centripetal force, or acceleration toward the center, would also vary directly as the distance from

[1] Estimates by Kelvin made the density somewhat less than that found here. See *Popular Lectures*, Vol. I, p. 419.

the center. In this case the equality of the two forces would occur throughout the whole equatorial plane simultaneously and there would be no rings formed with subsequent contraction of the interior parts. If the mass were flattened at the poles, and especially if the density increased towards the center, the centripetal force would not increase so rapidly as *directly as the distance from the center*, and the centrifugal force and the acceleration toward the center would, therefore, become equal first for the extreme periphery. Since both of these conditions must be supposed to have been fulfilled under the hypothesis, and since there cannot have been any appreciable cohesion, the conclusion is that *the matter would have been left behind continually after the process was started, and that there could have been no separate rings formed*. This conclusion is equally certain whether we suppose the nebula to have been gaseous or meteoroidal.

There does not seem to be a remote chance for the separation of a large mass by fission, and it may be in order to remark that none of the conclusions arrived at by the discussion of the figures of equilibrium of rotating *homogeneous fluids* can lay any claim whatever to exactness when *heterogeneous gases* are under consideration.

8. *On the contraction of a ring into a planet.*—It will now be assumed, without regard to fact, that a Laplacian ring has been left off and that the material of which it was composed has nearly all been gathered into a planet. It will be assumed, further, that the planet revolves in a *circle* around the Sun, and that the portion of the ring which remains is composed of infinitesimal masses neither disturbing each other nor affecting the motion of the planet. Neglecting the perturbations arising from the attractions of the other planets it is required to determine, so far as is possible, whether the infinitesimal remnant of the ring will be precipitated upon its planet or not. This problem is chosen because of its relative simplicity, and because it seems to be the case most favorable to aggregation.

Take the center of gravity of the planet and Sun as the origin, and refer the motion of the infinitesimal bodies to

rectangular axes rotating with the angular velocity with which the planet moves. Choose the origin of time so that the positive end of the x-axis shall pass through the planet. Let M represent the mass of the Sun, and m that of the planet, and choose the unit of mass so that $M+m=1$. Choose the unit of distance so that the radius of the planet's orbit shall equal one, and the unit of time so that the Gaussian constant shall equal one. Then it follows that the angular velocity of revolution of the planet is also equal to unity. The coördinates of M and m are respectively the constants x_1, 0, and x_2, 0. Consider one of the infinitesimal bodies and let its coördinates be x, y, and its distances from M and m, R and r respectively. Then the differential equations defining the motion of the infinitesimal body with respect to the rotating axes are

$$\left.\begin{aligned} \frac{d^2x}{dt^2} - 2\frac{dy}{dt} &= x - \frac{M(x-x_1)}{R^3} - \frac{m(x-x_2)}{r^3}, \\ \frac{d^2y}{dt^2} + 2\frac{dx}{dt} &= y - \frac{My}{R^3} - \frac{my}{r^3}. \end{aligned}\right\} \tag{1}$$

From these equations the integral

$$\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 = x^2 + y^2 + \frac{2M}{R} + \frac{2m}{r} - C \tag{2}$$

is found, in which C is the constant of integration. This equation is the relation between the coördinates and velocities which will always be fulfilled, because it has been assumed that there are no perturbations. (It is probable that in the long run the perturbations of the other planets already formed would neither assist nor hinder the process of agglomeration.) In particular, it defines the curves at which the velocity of the infinitesimal body will always be zero and which it can never cross.[1] The equation of the curve is obtained from (2) by putting the right side equal to zero, and is

$$x^2 + y^2 + \frac{2M}{R} + \frac{2m}{r} = C. \tag{3}$$

[1] See Hill's memoir on The Lunar Theory, in the *American Journal of Mathematics*, Vol. I, and Darwin's memoir on Periodic Orbits, in the *Acta Mathematica*, tome 21.

It is necessary to assign particular values to M and m in order to obtain numerical results. For the sake of rendering the computation easy it will be supposed that $M = \frac{4}{5}$ and $m = \frac{1}{5}$. Then let particles be taken in different parts of the Laplacian ring, as at A_1, A_2, A_3, A_4, and A_5, and suppose they move in the same direction and with the same velocity as the planet. They define in succession the constant C, and the corresponding curves of relative velocity are given in the figure and denoted by C_1, C_2, C_3, C_4, and C_5, respectively.

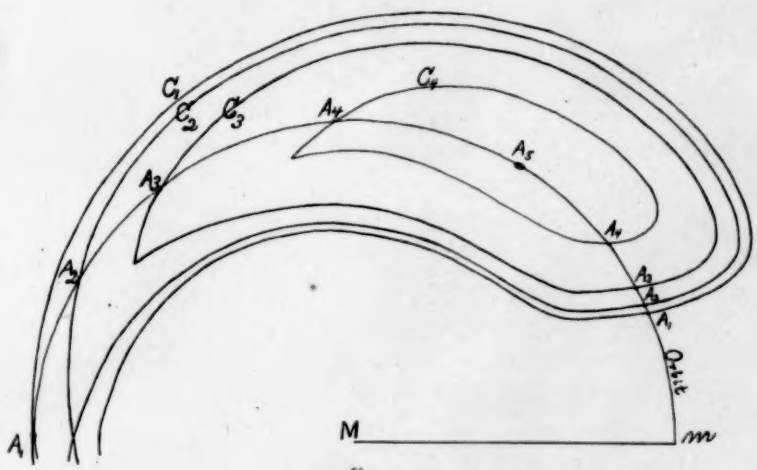

FIG. 1.

It must be borne in mind that the system is referred to rotating axes so that the Sun and planet have constant coördinates. Only the upper half of the figure is given, as the lower part is similar and oppositely placed.

The particle starting at A_1, on the left cannot get to the planet without passing *entirely around* one side or the other of the horseshoe, which is the region of imaginary velocities. This investigation does not show whether it will actually recede to a sufficiently great distance from the circle to pass around this forbidden region, but it seems very improbable that it will do so. On the other hand, there is no apparent reason why the particle at A_1 on the right cannot move along the circle to the planet.

For the initial conditions A_2 the horseshoe is on the point of dividing at the toe, and the limitations upon the motion of the infinitesimal body are similar to those in the case of A_1. There is a difference in the case of A_3 on the left, for the infinitesimal body may get to the planet by passing around the horseshoe, or by passing through the opening at its toe. There is no hindrance to A_3 on the right. Precisely similar remarks apply to the particles at A_4, the differences being evident from the figure.

The horseshoe vanishes at the point A_5, and the particle may move in any direction so far as the curves of zero relative velocity are concerned. But this is the point which forms an equilateral triangle with the Sun and planet, and is one of the Lagrangian solutions of the Problem of Three Bodies. The particle will, therefore, always remain at that relative point unless disturbed by forces other than those considered.

From the considerations advanced here it seems probable that the planet could gather up material lying on the circle in which it moves, and extending in each direction to the points of the equilateral triangular Lagrangian solutions; beyond these points the conditions seem to be adverse to the aggregation of the particles of the ring with the planet. Thus, if it is supposed that the evolution has already advanced until only an infinitesimal ring remains, *it seems extremely improbable that it could complete itself except for an arc of the circle extending 60° on each side of the planet.* These results are not quite so conclusive as might be desired; but the excuse for their appearance is that they are the first arrived at in a subject fraught with great difficulties, and that they show in a decisive manner that the development of a planet out of a Laplacian ring is not so simple a matter as has been generally supposed. If the mass of the planet were taken very much smaller than the Sun, as is the actual case in the solar system, its power of gathering up the ring would be less; but on the other hand, the region of imaginary velocities would be much thinner.

9. *On the starting of a planet from a ring.*—A question of as great importance as the one which has just been considered, is

whether any condensation could begin in a ring of tolerable homogeneity. The ring as a whole would have as a resultant attraction upon any particle, a force acting in a line passing through the center of the Sun. Moreover, the intensity would be small and the same for all particles at equal distances from the center. In short, the conditions for equilibrium which have been found by Mme. Kowalewsky and M. Poincaré in the discussion of *Saturn's* rings, would be approximately verified, and there would be no sudden accumulation of material at any one point. It seems that the condensations would have their immediate causes in local irregularities in the distribution of the matter.

The first question is naturally whether bodies of considerable size would not rush together as a result of their mutual attractions. If the velocities were such as they would be when the nebula is in convective equilibrium Darwin found in his memoir, "On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony," in 1888, that two bodies with a mean density of 10 would need to have masses one twentieth of that of the Earth in order that their attractions might deflect the directions of their motions by 10° when they passed near enough to each other just to graze.

It may be supposed that the particles are moving in parallel directions with velocities depending upon their distances from the center of the Sun, as in undisturbed motion. In order to get an idea of what may happen, it will be well to investigate the amount of acceleration that small masses exert upon each other. For this purpose the following problem will be solved: Having given two spheres with diameters of 10 feet each and specific gravity 10; it is required to find how long it will take them to come together, when they are started from rest 100 feet apart, and are subject to no forces except their mutual attractions. Each one would weigh 164 tons at the surface of the Earth, and each has to move only 45 feet, yet the appropriate computation shows that it would take them 11.66 hours to come together. This illustrates the extreme feebleness of the force of gravitation between small masses, and shows that

two bodies of the maximum size that could be supposed to exist in the nebula could not unite in consequence of their own gravitation unless they were moving in orbits at very nearly the same distance from the Sun. As bodies of any considerable size would occur at very rare intervals under the Laplacian hypothesis, and as very particular initial conditions would be necessary in order that collision might ensue, it is seen that the union of two bodies by mutual attraction would be a comparatively rare event, and that the relative velocity at impact would be very small. If for other reasons they collided with high velocities there would be danger of fracture; and if with low, there would be at least a slight rebound. It cannot be supposed that there would be much cohesion or adhesion between two bodies just having come into contact.

Moreover, the disturbing action of the Sun would have been very marked in the tenuous masses which would have existed after separation, and it is conceivable that these forces would have been sufficient to have disrupted a globular body of the density which must have prevailed if it had been brought to within planetary distances from the Sun. This possibility emphasizes the desirability of making an investigation, more refined than a mere guess, in regard to whether a fluid of this low density could have maintained itself by the gravitation of its parts in a continuous figure of equilibrium. This question will be considered in the next section.

10. *On the application of Roche's limit and a new criterion of a somewhat similar character.*— It is doubtless true that a fluid mass subject to disturbances will tend to assume the figure of most stable equilibrium. It is implied in the nebular theory that this figure is the one which is most nearly spheroidal in form; for, otherwise, the rings would have remained as they were or developed into something different from the planets. It will be shown in this section, by the application of Roche's limit and a certain new criterion, that these nearly spherical forms of equilibrium would not be possible at the respective distances of the planets for fluids so rare as the rings must certainly have been.

It will be inferred then that the rings could not have contracted into these forms without first having become very much condensed.

The rings, when first separated, must have been of the same density as the solar nebula near its limits; for at the instant before separation they constituted a part of it, although exerting no pressure on the surface where division was about to take place. Considering the extreme tenuity of the material at the planetary distances, it cannot be supposed that there were any violent changes in form or density at the time of separation, as there could have been no appreciable cohesion. The objections about to be advanced will apply with equal force if the genesis of the planets is supposed to have been by the fission process.

Roche's limit.— It has not been possible to obtain a complete exposition of Roche's theory, so there is some uncertainty in regard to the precise conclusions reached by him. From the more or less popular accounts of his work, and from that given in Tisserand, tome II, chap. 8, it is inferred that Roche proved that a certain ellipsoid with unequal axes is a figure of equilibrium for an incompressible fluid, revolving in a circle around a spherical mass; that the figure is stable if the fluid body is at a sufficiently great distance from the other; that if the densities of the two bodies are equal, this limit is 2.44 times the radius of the central body, and that it varies inversely as the cube root of the density of the revolving fluid.

If the solar nebula were homogeneous, its density would vary inversely as the cube of its radius. If it is supposed that there was a pronounced condensation toward the center, as must certainly have been true, its density in the outer regions would be less than that found by computing it on the hypothesis of homogeneity. On the other hand, as has just been seen above, the inferior limit of density for the existence of Roche's figure varies inversely as 2.44 times the cube of the distance. Therefore the density of the rings must have been less than $\frac{1}{2.44}$ of that necessary for the equilibrium of the ellipsoidal figure. Neither the whole mass nor any portion of it could have assumed

the ellipsoidal figure unless there had been a heterogeneity giving local regions of very much greater density than the average. This, however, is contrary to the nebular theory as we have defined it.

A new criterion.—It is extremely difficult to express physical problems with perfect rigor in mathematical terms. It is almost invariably true that certain approximate conditions have to be replaced by others which are purely ideal. It does not matter then with what rigor the deductions are carried out, the conclusions can have no greater approximation to the truth than the idealized conditions do to the actual.

In the derivation of Roche's limit the assumption was made that the satellite was a perfectly homogeneous incompressible fluid, and that its rotation and revolution were performed in the same period. It is certain that the first assumption is not valid in dealing with gaseous bodies, and it can scarcely be considered as being approximately true. The problem of determining the figures of equilibrium of compressible gases in motion lies in an untouched field, and is evidently one of very great difficulty. Even in the case where the fluid has been assumed to be homogeneous, the results obtained have been of the nature of shrewd guesses which have been verified by showing that they fulfill the sufficient conditions. The difficulties in solving the equations for equilibrium arise from the fact that the relations which must be fulfilled involve the form of the body implicitly, since they depend upon the attraction of the body for all points upon the surface.

The method employed here will be to shift the point of weakness. Roche made the violent assumption of homogeneity of the fluid, and then verified, by the help of Lagrange's investigations of the attractions of ellipsoids, that a certain ellipsoid would fulfill the conditions for equilibrium; in this work the law of density is not specified, but the law of attraction for points throughout the fluid mass is assumed. The forms of equilibrium are then determined, and the limiting cases in which the bodies cease to have closed surfaces.

It is certain that the bodies will be densest in their central portions, and that the surfaces of constant density will become more nearly spherical as the center is approached, and it seems probable that the bodies as a whole will be more nearly spherical than they would be if they were homogeneous.

For the sake of simplicity in the discussion, it will be assumed that the attractions depend only upon the distances from the center of gravity of the mass. This is equivalent to assuming, in the computation of the forces arising from the mutual gravitation of the parts, that the masses are made up of homogeneous concentric layers. Since these forces are feeble compared to those arising from the revolution of the fluid and from the attraction of the central mass, the errors introduced will be relatively unimportant. The law of attraction upon the different parts will depend upon the distribution of the mass, and will vary between that given for a homogeneous body and that in which the mass of the outer regions is infinitesimal compared to that of a central core. If the body were homogeneous the attraction would vary directly as the distance from the center, which is one extreme, and is nearly that considered by Roche. If the finite part of the mass were all condensed into a central core the attraction would vary inversely as the square of the distance from the center. This is the other extreme, and the limits will be developed under this assumption.

Let M represent the mass of the central body and m that of the fluid satellite. Choose the unit of mass so that $M + m = 1$. By hypothesis they describe circles around their common center of gravity. Take the origin of the coördinates at the center of gravity of the fluid satellite, and let the negative end of the x-axis pass through the center of gravity of the large body. Choose the unit of length so that the distance of the two bodies apart shall equal unity, and the unit of time so that the Gaussian constant shall equal one; then it follows from the theory of circular motion that the angular velocity of revolution is also unity. Let X, Y, and Z be the sums of the resolved components of the accelerations along the x, y, and z-axes respectively.

Then if the body is in equilibrium the fundamental hydrodynamical equation

$$Xdx + Ydy + Zdz = 0 \tag{4}$$

must be fulfilled for every point of the surface. Let the distances from any point to M and m be denoted by R and r respectively. Then with the units chosen it is easy to verify that the following are the expressions for X, Y, and Z.

$$\left.\begin{aligned} X &= M + x - \frac{mx}{r^3} - \frac{M(1+x)}{R^3}, \\ Y &= \quad y - \frac{my}{r^3} - \frac{My}{R^3}, \\ Z &= \quad - \frac{mz}{r^3} - \frac{Mz}{R^3}. \end{aligned}\right\} \tag{5}$$

Substituting in (4) and integrating we find

$$x^2 + y^2 + 2Mx + \frac{2m}{r} + \frac{2M}{R} = C. \tag{6}$$

This equation defines the *surfaces de niveau*, or level surfaces. The fluid body will be in equilibrium as long as the small fold around its center of gravity is a closed surface; but when this fold unites with others which are exterior, the body will begin to disintegrate. Therefore, the question of the equilibrium of a fluid mass which is very dense at its center is settled by the form of the surface defined by equation (6).

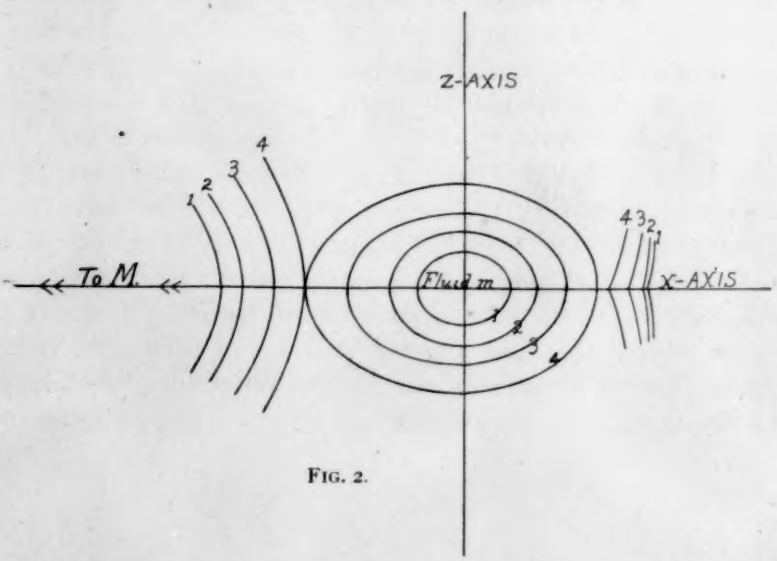

FIG. 2.

For brevity, the detailed discussion of the surfaces will be omitted. The conclusion is that for sufficiently large values of C they consist of a closed fold somewhat ellipsoidal in form around each of the bodies M and m, and of an approximate cylinder whose axis is parallel to the z-axis and passes through the center of gravity of the whole system. The following sections of the xz and xy-planes with the surfaces give a clear idea of their form.

It is seen that the oval around the center of gravity of m will have its first contact with exterior folds in the xy-plane, and even on the x-axis. At the point where instability sets in the pressure at the surface becomes zero. If we express (6) in terms of r and the polar angles, and write it $F(r, \phi, \theta) = C$, then the condition for zero pressure is $\frac{\delta F}{\delta r} = 0$. This point will occur first on the x-axis between m and M; therefore y and z are zero, and the point is found by taking the derivative of (6) with respect to r, and setting the result equal to zero. Performing these operations and clearing of fractions we obtain

$$r^5 - (2 + M) r^4 + (1 + 2M) r^3 - m r^2 + 2m r - m = 0. \qquad (7)$$

In all the applications which will be made m will be very small compared to M, and the value of r satisfying (7) will consequently also be small. Then a sufficiently approximate solution of (7) is

$$r^3 = \frac{m}{(1 + 2M)}. \qquad (8)$$

Let σ represent the mean density of the mass m; then the condition that its radius shall just equal r, as defined by (8) is

$$r^3 = \frac{3m}{4\pi\sigma} = \frac{m}{(1 + 2M)}, \qquad (9)$$

or solving

$$\sigma = \frac{3(1 + 2M)}{4\pi}. \qquad (10)$$

Since m is very small, M is very nearly equal to unity, and $\sigma = \frac{9}{4\pi}$, approximately.

Let σ_0 represent the mean density of the solar nebula. Then in the units chosen

$$\sigma_0 = \frac{3}{4\pi}.$$

Therefore, $\sigma_0 = \frac{\sigma}{3}$ and the mean density of the fluid mass must have been three times as great as that of the solar nebula when they separated in order that this figure of equilibrium might

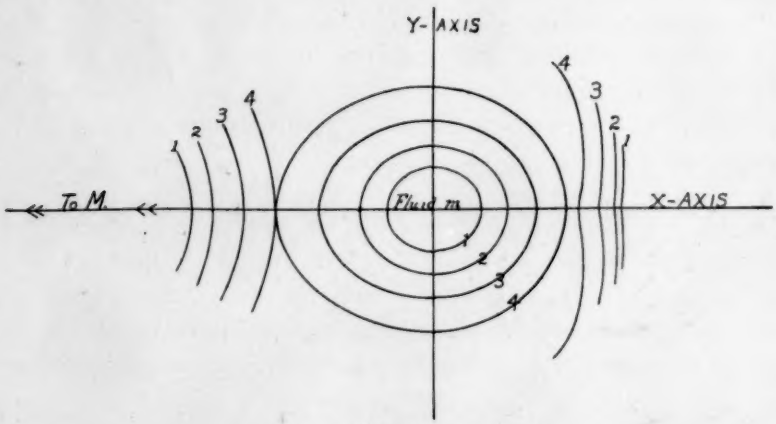

FIG. 3.

have been stable. A simple computation shows that if m and M have the same density, then m must be distant at least 1.35 times the radius of M from its center of gravity in order that it may be in stable equilibrium. The discussion of these limits shows that a Laplacian ring could not have contracted into a planet, and that the condition of the solar nebula must have been one of great heterogeneity instead of homogeneity in concentric layers.

III. A METHOD OF THE THIRD CATEGORY.

11. *On the moment of momentum of the solar system.*—It is known from the elementary principles of dynamics that the moment of momentum of a system which is subject to no external forces is constant. We shall have occasion in this section to compute the moments of momenta of the solar system at

different points of its supposed development. If a sphere and an oblate spheroid having equal masses and equal equatorial radii are composed of the same material and rotate in the same period, then the moment of momentum of the oblate spheroid will be the greater; for the greatest difference between it and the sphere is in the polar regions where the effect upon the moment of momentum of the displacement of matter is small, while it must be denser in the equatorial regions to preserve the equality of masses, since it does not seem in the least probable that a rotating body would be denser at the center than the same body would if it were stationary. Therefore, if we compute the moment of momentum which the solar nebula had when it extended to *Neptune's* orbit under the supposition that it was spherical in form we shall get a result which is too small. The moment of momentum of the present system can be determined with almost perfect accuracy, because the planets present no difficulties while the Sun is nearly an exact sphere.

Let M represent the moment of momentum of a sphere of the radius R, rotating with the angular velocity ω. Let σ represent the density; then we have

$$M = \omega \int_0^{\pi} \int_0^{2\pi} \int_0^{R} \sigma \, r^4 \sin^3\theta \, d\theta \, d\phi \, dr . \qquad (11)$$

Suppose the density depends upon the distance from the center; then $\sigma = f(r)$. Substituting in (11), and integrating, we have

$$M = \frac{8}{3} \pi \omega \int_0^{R} f(r) \, r^4 \, dr . \qquad (12)$$

The law of density of a gaseous sphere has been determined by Ritter in a paper in Wiedemann's *Annalen* (New Series) Vol. XVI, 1882, p. 166, entitled "Untersuchungen über die Höhe der Atmosphäre und die Constitution gasförmiges Weltkörper." The problem has also been treated by Dr. G. W. Hill in a paper in the *Annals of Mathematics*, Vol. IV, 1888. Professor Darwin has given a complete discussion of the same problem in a memoir read before the Philosophical Society November 15, 1888, "On the Mechanical Condition of a Swarm of Meteorites, and on the

Theories of Cosmogony." The law of density used in the following computations is that of Darwin's "Isothermal-Adiabatic" sphere given in *loc. cit.* p. 25. The density is given there for different values of the radius and agrees substantially with the results found by Ritter and Hill. We have computed the integral of (12) by a somewhat rough quadrature. The results are the following:

When the nebula extended to *Neptune's* orbit,	$M = 32.176$
When the nebula extended to *Jupiter's* orbit,	$M = 13.250$
When the nebula extended to the Earth's orbit,	$M = 5.690$
When the nebula extended to *Mercury's* orbit,	$M = 3.400$
In the system at present, - - - - -	$M = 0.151$

Instead of being a constant, the moment of momentum is found to vary in a remarkable manner. On account of the approximations made the first number is somewhat too small while the last is too large, as the Sun was assumed to be homogeneous in computing the moment of momentum which enters into it. Notwithstanding these errors in opposite directions, the moment of momentum in the first case is 213 times that in the last. It follows from these figures that if the mass of the solar system filled a spheroid extending to *Neptune's* orbit, and rotated with a velocity sufficient to make its moment of momentum equal to that of the present system, and if it then contracted with the law of density always that adopted above, the centrifugal force would not equal the centripetal until it had shrunk far within *Mercury's* orbit. Such an enormous difference cannot be ascribed to uncertainties in the law of density or to the approximations in the mechanical quadratures; but it points to a mode of development quite different from, and much more complicated than, that postulated in the nebular theory under discussion.

12. *Conclusions.*—It has been the purpose of this paper to gather up and to present in a connected form the greater part of the dynamical considerations bearing upon the nebular hypothesis which we have thus far been able to discover. In attempting to outline the whole problem in the limits of one paper the discussion of many points has been left in an incomplete state,

yet it is believed that the method of attack has been sufficiently clearly indicated, and that the conclusions are plain. It is believed that the nebular hypothesis treated is substantially the one held by astronomers to be true, and the one which has served as a basis for all the computations regarding the heat that the Sun has radiated and the age of the solar system.

Under the methods of the first category certain phenomena are enumerated which contradict the hypothesis so flatly that candid minds must admit that its validity in the form considered is open to serious question. In less exact sciences such objections would overthrow a theory or lead to its reconstruction. The objections are that the planes of the planets' orbits present considerable deviations, while four satellites revolve in planes making practically right angles with the average of the system; that the distribution of mass in the planets is unaccountably and suspiciously irregular; and that there is an unexplainable anomaly in the motion of the inner ring of *Saturn*.

Under the methods of the second category, it is shown that the development of a system of planets and satellites from an extended nebula is by no means a simple matter, and that in the system under consideration the conclusions which it was possible to make were invariably adverse to the theory. In subjects where perfectly rigorous mathematical processes cannot be employed such a uniform agreement of conclusions, when so various methods of attack are employed, is sufficient to establish a proposition. The objections are that the lighter elements would have escaped; that matter would have been left off continually instead of in rings at rare intervals; that if a ring were all contracted into a planet except an infinitesimal remainder distributed in its path, the process of aggregation could not complete itself; that the gravitation between the masses occurring in the rare media would be so feeble that they would seldom come in contact, and that Roche's limit and a similar new criterion show that fluid masses of the density which must have existed would be disintegrated by the disturbing action of the Sun.

The one objection which is advanced in the methods of the third category is of great simplicity and leads to certain conclusions. It is of such a character, and the numerical discrepancies are so great, that it seems to render the nebular hypothesis in the simple form in which it has usually been accepted absolutely untenable unless some fundamental postulates now generally accepted are radically erroneous. It seems a necessary inference from the results of the discussion that the solar nebula was heterogeneous to a degree not heretofore considered as being probable, and that it may have been in a state more like that exhibited in the remarkable photographs of spiral nebulæ recently made by Professor Keeler.

If the above conclusions are well founded, it follows that the age of the Earth computed from the theory of the Sun's contraction as a gaseous sphere arranged in homogeneous concentric layers can be accepted only with great reservation, and that it is folly to attempt to state what was the temperature of the Sun's surface when it extended to the orbits of the various planets. If it is established that the Laplacian hypothesis is only partially true, and that we do not yet know the precise mode of development of the solar system, geologists will feel freer to interpret geological phenomena in accordance with geological principles, and astronomers will have one of the most attractive fields of investigation still demanding their efforts for its complete exploration.

UNIVERSITY OF CHICAGO,
February 1900.

THE ORBIT OF THE SPECTROSCOPIC BINARY, χ *DRACONIS.*

By W. H. WRIGHT.

THE variable velocity in the line of sight of χ *Draconis* was announced by Professor Campbell in this JOURNAL (**6**, 291). Since July 1898 twenty-eight spectrograms have been secured with the Mills spectrograph, the variations in velocity meanwhile running through somewhat over a period. Data are therefore at hand for a fairly accurate determination of the star's orbit, and the computation has been assigned to the writer.

In the accompanying table the first column contains the numbers of the observations, the second the numbers of the plates, the third the dates of observation, the fourth the assigned weights, and the fifth the measured velocities.

	Plate No.	Date	Wt.	Vel. (observed)	Vel. (comp.)	O–C I	Vel. (comp.) II	O–C II
1	839 C	1898, July 25.8	1	+45.6	+44.0	+1.6	+44.7	+0.9
2	24 A	Sept. 5.7	1	46.0	46.8	−0.8	46.1	−0.1
3	51 B[1]	19.7	⅔	42.6	44.7	−2.1	43.2	−0.6
4	1038 A[1]	Oct. 26.6	1	14.5	14.3	+0.2	14.3	+0.2
5	52 A	Nov. 1.6	⅔	11 9	11.5	+0.4	11.6	+0.3
6	61 A	5.6	⅔	11.3	10.7	+0.6	10.9	+0.4
7	83 A	12.6	⅔	10.7	11.0	−0.3	11.0	−0.3
8	99 A	18.6	⅔	10.6	12.2	−1.6	12.1	−1.5
9	1122 A	Dec. 7.6	⅔	18.3	17.4	+0.9	17.4	+0.9
10	23 A	17.1	⅔	21.0	20.1	+0.9	20.2	+0.8
11	24 A	17.6	⅔	20.0	20.2	−0.2	20.3	−0.3
12	61 B	1899, Feb. 7.1	1	32.4	31.5	+0.9	32.2	+0.2
13	93 B	23.0	1	34.3	34.3	0.0	35.0	−0.7
14	1203 A	Mar. 7.0	1	36.7	36.2	+0.5	37.0	−0.3
15	09 D	Apr. 4.0	1	41.2	40.4	+0.8	41.1	+0.1
16	16 B	9.0	1	40.7	41.1	−0.4	41.8	−1.1
17	38 C	May 1.9	1	44.9	44.0	+0.9	44.5	+0.4
18	66 A	29.9	⅔	47.2	46.7	+0.5	46.6	+0.6
19	72 A	June 8.0	1	46.5	47.0	−0.5	46.6	−0.1
20	88 C	15.0	1	46.5	46.7	−0.2	46.1	+0.4
21	1310 D	27.9	⅔	42.7	44.6	−1.9	43.4	−0.7
22	19 A	July 5.7	1	41.5	41.4	+0.1	40.0	+0.5
23	26 D	11.9	1	35.8	37.1	−1.3	35.8	0.0
24	37 C	16.9	1	31.2	32.5	−1.3	31.5	−0.3
25	60 C	31.8	1	17.3	16.4	+0.9	16.9	+0.4
26	1417 B	Aug. 23.8	1	10.8	11.5	−0.7	11.4	−0.6
27	58 B	Sept. 11.7	⅔	17.5	16.6	+0.9	16.4	+1.1
28	1510 B	Oct. 16.7	⅔	26.4	25.6	+0.8	25.8	+0.6

[1] Measured and reduced by Professor Campbell.

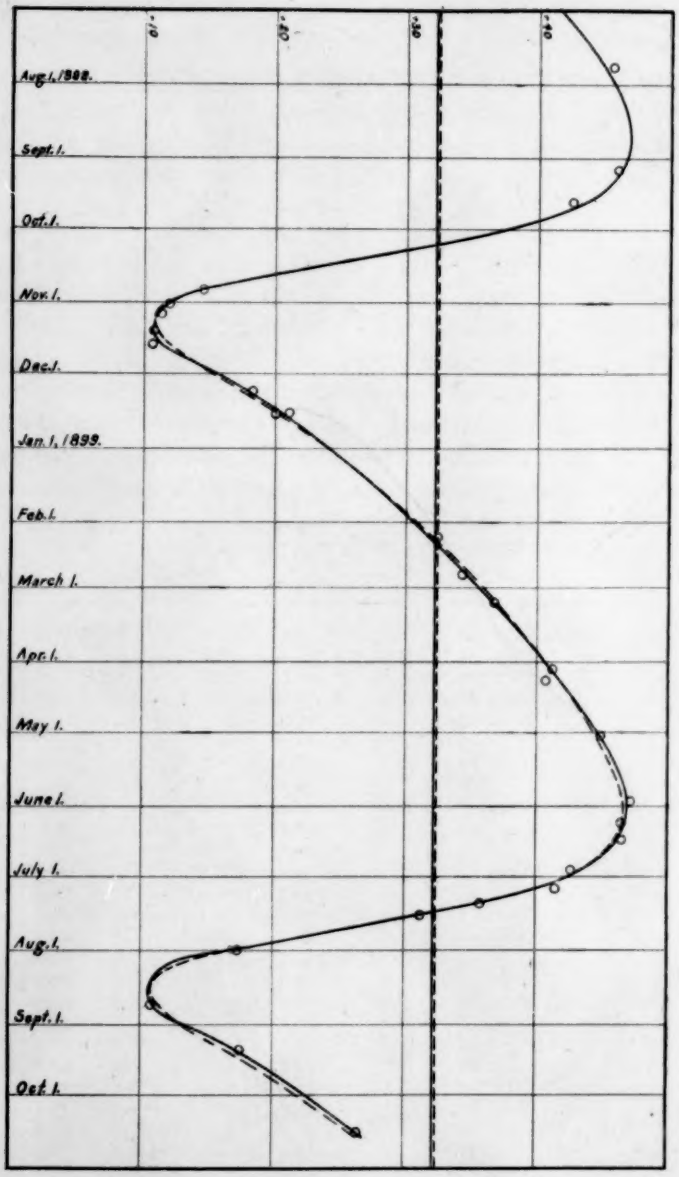
.10
.20
.30
.40
Aug. 1, 1898.
Sept. 1.
Oct. 1.
Nov. 1.
Dec. 1.
Jan. 1, 1899.
Feb. 1.
March 1.
Apr. 1.
May 1.
June 1.
July 1.
Aug. 1.
Sept. 1.
Oct. 1.

FIG. 1.

The observations of November and December 1898 were made necessarily directly after dark, a time very unfavorable for photographing spectra, on account of the rapid changes of temperature then occurring. They were accordingly given smaller weight than those made under normal conditions. In some other cases the observing conditions were very poor, wind, clouds, and bad seeing interfering with the work. In these also the weight was reduced.

The observations were platted in the usual manner, as shown in the accompanying figure, and through the points thus obtained the full-line curve was sketched as smoothly as possible. From this curve the following provisional elements were computed:

ELEMENTS. I.

$V = +\ 32.2$ km, velocity of system in line of sight.
$e = 0.45$, eccentricity of orbit.
$w = 114^{\circ}.99$, position of periastron.
$U = 281^{d}$, period.
$T = 1899$, July 27.0, time of periastron passage.

$$K = 18.15 = \frac{f}{\sqrt{p}} \sin i.$$

Using these elements as a basis, the velocities corresponding to the times of observation were computed, and are given in column 6 of the table. A least-squares adjustment of the elements was then undertaken. On account of their juxtaposition, the following observations were combined: (5–6), (7–8), (12–13–14), (15–16), (21–22); these, with the other observations used singly, gave twenty-one equations of condition. The form of the equations was that given by Lehmann-Filhés, except that the correction to the velocity of the system was introduced as an unknown. The following are the resulting elements:

ELEMENTS. II.

$V = +\ 32.38$ km $\pm$ 0.09 km.
$a \sin i = 62{,}020{,}000$ km $\pm$ 410,000 km.
$e = 0.423 \pm 0.006$.
$w = 119^{\circ}.0 \pm 1^{\circ}.1$.

$U = 281^d.8 \pm 0^d.7$.

$T = 1899$, July $28^d.3 \pm 0.5$.

$r_0 = \pm 0.4$ km per sec. probable error of a single observation of weight unity.

$[pvv]_{\text{elements I}} = 16.4$.

$[pvv]_{\text{elements II}} = 7.1$.

The dotted curve has been sketched in to represent these elements.

The spectrum of χ *Draconis* is very suitable for accurate measurement. It closely resembles that of *Procyon*. The $H\gamma$ line is well defined, and the metallic lines are exceedingly sharp and well separated.

LICK OBSERVATORY, UNIVERSITY OF CALIFORNIA,
December 1899.

A PRISM OF UNIFORM DISPERSION.[1]

By CHARLES G. ABBOT and FREDERICK E. FOWLE, JR.

IN many bolometric researches it would be extremely advantageous if there were some method of producing a spectrum on the normal or wave-length scale, and without attendant large losses or inequitable distribution of the energy of the source. It is well known that neither the grating nor the usual prism combines these three desiderata. Indeed, although the grating perfectly fulfills the first condition, in that it produces a normal spectrum, yet its fortuitously irregular distribution and great waste of radiations have generally prevented its use for bolometric purposes. Prisms have therefore been employed very largely in connection with the bolometer, and indeed also for most stellar spectroscopic researches, on account of the comparatively small losses of energy attending their use, although their dispersion varies enormously between the violet and red or infra-red ends of the spectrum.

It occurred to us, while comparing the dispersion curves of rock-salt and flint glass, that there might be a possibility of combining prisms of different dispersions and different angles in such a way as to give a spectrum in which equal increments of deviation should correspond approximately to equal increments of wave-length. Preliminary computations with a parallel sided combination of three prisms, of which the central one was rock-salt and the two outer ones glass, satisfied us that a nearly direct vision prism of these materials would be a very great improvement in uniformity of dispersion over either prism taken separately.

Recently ten small sample prisms of glass have been obtained from Mr. Brashear, and their dispersion curves have been roughly determined at this Observatory between the wave-lengths

[1] Published by permission of the Secretary of the Smithsonian Institution.

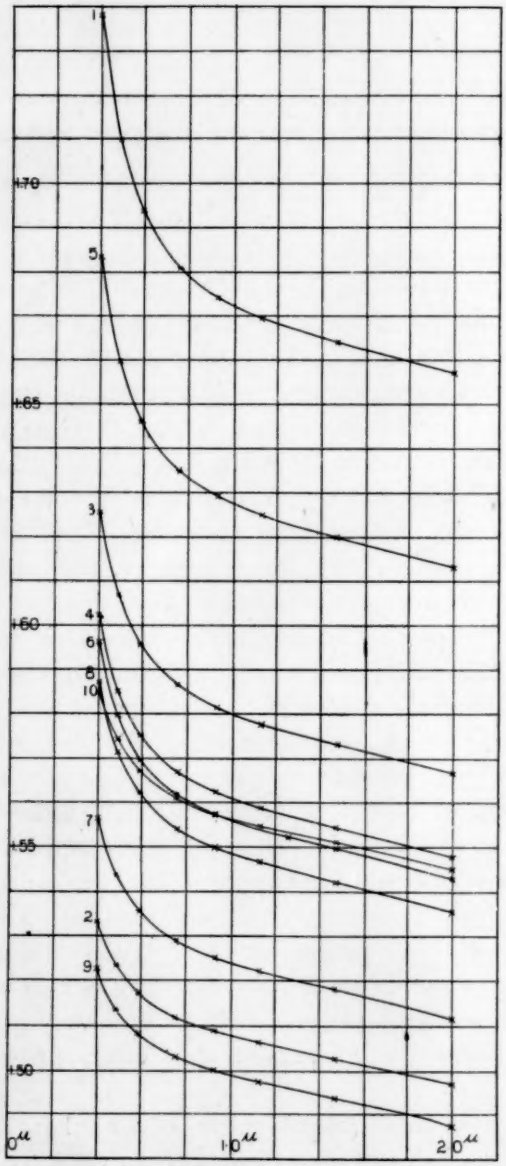

FIG. 1.

n and λ Curves of Glass Prisms.

0.4μ and 2.0μ. The n and λ curves are given in the accompanying illustration (Fig. 1).[1]

It will be seen that there is a gradual modification in form from first to last among the ten curves. In general the first point of inflection is in the red, and a second point of inflection at about 1.8μ has begun to appear in the later members of the series. The earlier curves show, both actually and relatively to the later curves, far greater dispersion in the visible than in the infra-red spectrum. Comparing the first prism with the last, we see that both have excessive dispersion in the visible spectrum, but the first has a far more excessive dispersion here than the last. Hence a small angle prism of the first kind opposed to a large angle prism of the last ought to correct this inequality approximately.

Computations were made for several such combinations, and it was found that very good results could be secured. Fig. 2 illustrates three arrangements of two prisms of 5° 10′ and 20° angle respectively, called prisms 1 and 9 in what follows. In the first case the beam *AAA* is assumed to enter the thin prism at zero incidence, thus emerging from the thick prism at an angle of emergence of about 22°. Next the beam *BBB* is supposed to enter the thick prism at about 22° incidence, emerging nearly normal to the face of the thin prism. Third the beam *CCC* enters the thick prism as in the second case, but is reflected at the last face of the thin prism (supposed silvered) and emerges nearly as it entered.

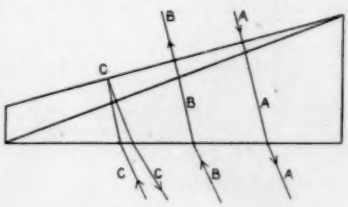

FIG. 2.

The results of these three arrangements, *A*, *B*, and *C*, are shown in curves *A*, *B*, and *C* of Fig. 3, and for the sake of comparison, curves *D* and *E* are added to show how much less uniform is the dispersion of either of the separate prisms. A

[1] The order of numbering conforms to a series of arbitrary numbers given to distinguish the prisms prior to investigating their dispersion.

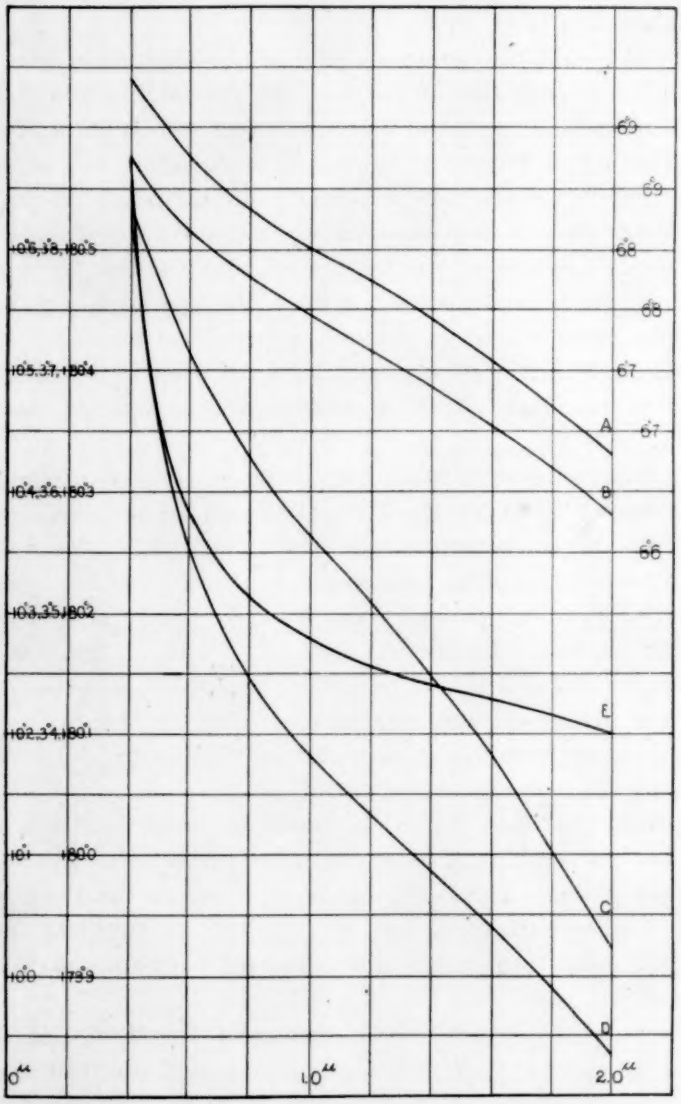

FIG. 3.
Deviation and Wave-Length Curves of Glass Prisms.

summary of the optical properties of these prisms and their combinations is given in the following table:

COMPARISON OF PRISMS.

Arrangement	Refracting angle	Angle of incidence	Mean deviation, θ	Total dispersion, $\Delta\theta$, from 0.4μ to 2.0μ	Limiting values of the dispersion $\frac{d\theta}{d\lambda}$	
					Maximum	Minimum
Prism 1	5° 10′	For minimum deviation	3° 37′	25′ 14″	180.0	3.3
Prism 9	20° 0′	" " "	10° 17′	43′ 30″	225.0	14.2
Combination *A*		00° 00′	6° 47′	18′ 30″	19.5	9.0
Combination *B*		21° 30′	6° 47′	17′ 34″	18.9	9.6
Combination *C*		21° 30′	180° 14′	36′ 50″	37.5	15.9

From an inspection of the table and the curves it appears that the combination *C* gives twenty-four times as close an approximation to uniformity of dispersion as prism 1 and seven times as close an approximation as prism 9. Long stretches of curve *C* might be chosen where the dispersion is practically uniform, and even throughout its whole extent from 0.4μ to 2.0μ the greatest dispersion is no more than twice the smallest. The convenience of the arrangement *C* for many researches leaves little to be desired, for the collimating mirror or lens could also be used to bring the spectrum to a focus, since the beam is practically returned upon itself.

In conclusion it may be said that while no general mathematical treatment of the problem of a uniform dispersion prism has been attempted by us, yet we have obtained what will be for many purposes, we hope, a sufficiently close approximation. It seems quite possible that objective prisms for photographic stellar work may be made in a similar way, so closely normal in dispersion through the limited range of wave-lengths employed as to require no correction whatever to compare the photographs with grating spectra.

ASTROPHYSICAL OBSERVATORY,
SMITHSONIAN INSTITUTION, January 1900.

THE VARIABLE VELOCITY OF β *HERCULIS* IN THE LINE OF SIGHT.

By W. W. CAMPBELL.

THE third plate of this star was measured in July 1899, and since the result differed 9 km from those given by the two previous observations, its velocity was assumed to be variable. Additional plates amply confirm the variation.

1897 June	1,	—32.	km. ±,	underexposed
1898 June	13,	—31.9,	measures by	Wright
1899 February	26,	—23.3,	"	" Campbell
July	31,	—30.9,	"	" Wright
August	21,	—30.8,	"	" Campbell
1900 January	10,	—21.0,	"	" Campbell

The period is undetermined, but it appears to be long.

NOTE.—I am indebted to Miss Clerke's interesting article in the *Observatory*, November 1899, p. 389, for a reference to the Harvard College Observatory's list of stars whose spectra are apparently composite. This portion of Miss Maury's work on the classification of star spectra was unfamiliar to me; otherwise, in my papers announcing the discovery of the variable velocities of o *Leonis*, β *Capricorni* and υ *Sagittarii*, I should have called attention to her recognition of the composite character of their spectra. Our collection of spectrograms includes two or more plates each of the majority of the stars on Miss Maury's list of composites; but no evidence of variable velocity has been detected except for the three stars mentioned above.

LICK OBSERVATORY,
January 11, 1900.

THE DETERMINATION OF THE MOON'S THEORETICAL SPECTROGRAPHIC VELOCITY.

By W. W. CAMPBELL.

INASMUCH as *Venus* and *Mars* are often inaccessible, it is sometimes desirable to obtain spectrograms of the Moon, in order to test the accuracy of radial velocity determinations. The Moon's apparent velocity in the observer's line of sight may on occasion amount to nearly two kilometers per second,[1] and is made up of several components, as follows:

1. The radial velocity V_1 of the Earth's center with reference to the Sun.

2. The radial velocity V_2 of the Moon with reference to the Earth's center.

3. The component V_3 of the velocity V_2 in the line joining the Sun and Moon.

4. The component V_4 of the Moon's orbital velocity (referred to the Earth's center), in the great circle passing through the Sun and Moon.

5. The component $-v_\phi$ in the line of sight of the observer's velocity due to the Earth's diurnal rotation. (The component of the Moon's rotational velocity with reference to the Sun will not exceed the half of one hundredth of a kilometer per second, and may be neglected.)

The values of these components may be computed quickly by the following methods, which have been developed with reference to utilizing data tabulated in the Nautical Almanac.

1. Let log D_1 be the logarithm of the radius vector of the Earth, and let a be the hourly change in log D_1. It can be shown by elementary methods[2] that the radial velocity of the Earth's center in kilometers per second is given by

$$V_1 = \frac{149{,}500{,}000}{\omega m} D_1 a = [4.9805]\, D_1 a \qquad (1)$$

[1] Among the first to call attention to this subject were Ranyard (in the *Observatory*, January 1892, pp. 39–40), and Deslandres (in *Comptes Rendus*, 120, 419, 1895).

[2] See paper on "The Mills Spectrograph," this JOURNAL, 8, 151–154.

The quantities D_1 and a are tabulated in the Nautical Almanac.

2. Let

R = the equatorial radius of the Earth in kilometers,
D_2 = the Moon's distance from the Earth's center,
p = the Moon's horizontal parallax, and
δp = the change in the parallax in 12 hours;

then

$$D_2 = \frac{R}{\sin p} = \frac{[3.8047]}{\sin p}, \qquad (2)$$

and

$$V_2 = \frac{\delta D_2}{\delta t} = -\frac{R \sin 1''}{3600} \cdot \frac{\cos p}{\sin^2 p} \cdot \delta p = -[4.934] \operatorname{cosec}^2 p \cdot \delta p \,. \qquad (3)$$

The quantities p and δp are tabulated in the Nautical Almanac.

3. Let E be the angular distance between the Sun and Moon. Then

$$V_3 = -V_2 \cos E. \qquad (4)$$

The "lunar distance" E is tabulated in the Nautical Almanac.

4. Let δE be the variation in E in 1 second,—being positive or negative according as the distance is increasing or decreasing. Then

$$V_4 = \sin 1'' \, D_2 \sin E \, . \, \delta E = [4.6856] \, D_2 \sin E \, . \, \delta E \,. \qquad (5)$$

The value of D_2 is obtained from equation (2). The lunar distance E and the arithmetical complement of log (δE) are tabulated in the Nautical Almanac.

5. The value of $-v_d$ is given by[1]

$$-v_d = [9.672] \sin t \cos \delta \cos \phi \,. \qquad (6)$$

The apparent velocity of the Moon in the line of sight is given by

$$V = V_1 + V_2 + V_3 + V_4 - v_d. \qquad (7)$$

Example.—The Moon was observed with the Mills spectrograph at

Mt. Hamilton sidereal time, 1900, January $9^d\ 14^h\ 30^m$;
Greenwich mean time, " " 9 17 20.

[1] See *Astronomy and Astro-Physics*, 11, 319.

Determine the Moon's theoretical spectrographic velocity at this time.

(1)

Constant =	4.980
$\log D_1$ =	9.993
$\log a$ =	3.820
$\log V_1$ =	8.793
V_1 =	+ 0.06 km

(2)

Constant =	3.805
$\sin(57' 27'')$ =	8.223
$\log D_2$ =	5.582

(3)

Constant =	4.934
$\text{cosec}^2(57' 27'')$ =	3.554
$\log(-1.45)$ =	0.161_n
$\log V_2$ =	8.649
V_2 =	+ 0.04 km

(4)

$\log V_2$ =	8.649
$\cos 114.^\circ 7$ =	9.621_n
$\log V_3$ =	8.270
V_3 =	+ 0.02 km

(5)

Constant =	4.686
$\log D_2$ =	5.582
$\sin 114.^\circ 7$ =	9.958
$\log(\delta E)$ =	9.709
$\log V_4$ =	9.935
V_4 =	+ 0.86 km

(6)

Constant =	9.672
$\sin 26.^\circ 5$ =	9.650
$\cos 19.1$ =	9.975
$\cos 37.3$ =	9.901
$\log(-v_d)$ =	9.198
$-v_d$ =	+ 0.16 km

(7)

V_1 =	+ 0.06 km
V_2 =	+ 0.04
V_3 =	+ 0.02
V_4 =	+ 0.86
$-v_d$ =	+ 0.16
V =	+ 1.14 km

Measures of this spectrogram by two observers gave, as the mean observed velocity, + 1.46 km.

LICK OBSERVATORY,
January 18, 1900.

A PROPOSAL FOR THE PHOTOMETRIC OBSERVATION OF THE PLANET *MERCURY* DURING THE TOTAL SOLAR ECLIPSE ON MAY 28, 1900.

By G. MÜLLER.

DURING the years 1878–1888 I observed the planet *Mercury* photometrically as often as its visibility in the morning and evening twilight made this at all possible, and I deduced from these observations with sufficient accuracy the connection between its brightness and the magnitude of the phase for phase-angles from 50° to 120°. The discussion of these observations showed that for the range of phase in question the light-curve of *Mercury* is almost absolutely identical with the light-curve found by other observers for the Moon. It further appeared that, if we may conclude as to the brightness of the planet at smaller phase-angles from the portion of the curve investigated, the behavior of *Mercury* is very similar to that of a number of the minor planets, and that therefore approximate values of the diameters of these small bodies could be deduced on the assumption of an equal albedo.

These conclusions, which are not without interest in themselves, could be subjected to a considerably sharper test if it should be possible to determine directly the brightness of *Mercury* at phase-angles between 0° and 50° by photometric measurements. At other stations more favorably situated than the Potsdam Observatory in respect to the purity of the air, as, for instance, at mountain stations, and perhaps also in more southern latitudes, it would certainly be possible to follow *Mercury* closer to the conjunctions than I was able to do; but under any circumstances it could only be a question of the gain of very few degrees of the phase-angle, and it would always be necessary to take into the bargain the disadvantage that the observation would have to be made at very low altitudes.

But as I have already indicated in my memoir on the Brightness of the Planets (*Potsdamer Publicationen*, Band VIII, p. 311), total solar eclipses under some circumstances offer an excellent opportunity to fill out the gap in the light-curve of *Mercury*, and to observe the planet photometrically during the daytime, and even when it is high above the horizon. In this respect the approaching total solar eclipse of May 28 will be especially favorable, and I therefore desire to respectfully recommend that this opportunity should not be allowed to pass unutilized.

The eclipse will be visible in the southern part of North America, in Portugal, Spain, and North Africa. According to the Nautical Almanac, *Mercury* reaches superior conjunction with the Sun on May 29, at 19^h G. M. T. The following values are computed for the phase-angle:

May 28, 0^h G. M. T. $7^\circ.3$
May 29, 0^h G. M. T. $3^\circ.6$

Totality will begin at $1^h\ 32^m$ G. M. T. for an observing station on the eastern coast of America, and at $4^h\ 2^m$ G. M. T. for a place on the coast of Portugal. The phase-angle of *Mercury* therefore amounts to 7° at the eclipse, and its angular distance from the Sun to something over 2°.

As to the brightness of the planet, this (expressed in stellar magnitudes) may be computed, according to my investigations, between the range of phase from 50° to 120° for any moment, according to one of the two following formulae:

$$\text{(I)}\quad h = -1.041 + 5(\log r + \log \Delta - \log r_0) + 0.03679(\alpha - 50)$$

$$\text{(II)}\quad h = -0.901 + 5(\log r + \log \Delta - \log r_0) + 0.02838(\alpha - 50) + 0.0001023(\alpha - 50)^2,$$

where α is the phase-angle, r and Δ the appropriate distances of the planet from the Sun and Earth, r_0 is the mean distance of *Mercury* from the Sun. If the formulae are applicable for smaller phase-angles than 50°, we obtain for the brightness of *Mercury* at the time of the total eclipse the value —2.5 according to formula (I), and —1.8 magnitudes according to formula (II).

The brightness would therefore be just about the same as that of the planet *Jupiter* at its mean distance.

Venus is the most suitable object for comparison, being about 40° east of the Sun at the time of the eclipse. Her angle of phase is 113°, and she happens to be exactly at her greatest brilliance (brightness $= -4.2$ magnitudes), and she can be perceived with the naked eye in full sunlight.

The question might be raised whether the short duration of totality, being only $1^m\ 45^s$ on the eastern coast of America, and only $1^m\ 33^s$ in Portugal, is sufficient for a precise determination of brightness. To this may be replied that inasmuch as *Venus* can be observed both before and after totality in consequence of her favorable position, the whole duration of totality can be employed for measurements on *Mercury* alone. A practiced observer, however, is easily able to make from four to six settings of the photometer in one and a half minutes, which are quite sufficient for furnishing an accurate measurement of the brightness. During the day previous to the eclipse the most favorable aperture for the observation of *Venus*, as well as the necessary shade glasses, may be tested, and since the difference of brightness to be measured between *Venus* and *Mercury* amounts to about two magnitudes, the conditions may be realized in advance under which the measurements must be carried out during the eclipse. An accurate value of the brightness of *Venus* can be made, if necessary, on the days before and after the eclipse by comparison with bright stars. It would be advisable to employ small objectives of very short focus, as I have always done in my observations of planets, in order to obtain images of the planets in the form of points; it is also desirable to employ only those photometers with which, as in the case of Zöllner's, the effect of the different brightness of the background of the sky is eliminated.

Unless entirely unexpected difficulties should arise, I feel convinced that it should be possible to obtain a thoroughly reliable determination of the brightness of *Mercury* during the eclipse. I hope myself to be in a position to make an attempt of this sort at a station in Portugal; but as this experiment might

possibly fail on account of unfavorable weather, or for other reasons, I beg to direct the attention of those astronomers who intend to observe the approaching eclipse — and especially those who are experienced in photometric measurements — to the problem here proposed, and to request their kind coöperation in order that so favorable an opportunity for extending our knowledge of the conditions of illumination of the planets may not be unutilized.

POTSDAM, ASTROPHYSIKALISCHES OBSERVATORIUM,
February 8, 1900.

ON THE LAW OF DIURNAL ROTATION OF THE OPTICAL FIELD OF THE SIDEROSTAT AND HELIOSTAT.

By M. A. CORNU.

THE well-known instruments known as the *Heliostat* and the *Siderostat* permit a beam of light coming from a heavenly body sharing the diurnal motion to be sent in a constant direction by means of a movable mirror. The geometrical theory of these instruments is very simple; the heavenly body is considered as a luminous point and the incident beam is regarded as a straight line which in twenty-four hours describes a cone of revolution around the polar axis of the instrument, which is itself parallel to the Earth's axis. In order to secure the fixity of the reflected beam it is necessary and sufficient that the normal to the mirror shall remain parallel to the bisector of the angle between the ray coming from the heavenly body and a given fixed direction. This is the condition which is more or less perfectly realized in the instruments invented by S'gravesande, Gambey, Silbermann, Foucault, and others.

If the beam reflected by the mirror thus guided is received by a telescope along its principal axis, the focal image of the heavenly body will remain fixed in the center of the field of view in spite of the angular displacement of the celestial sphere. But this condition of fixity, geometrically realized for the object under examination, is no longer fulfilled for the neighboring regions: it is easily shown that the field of view turns about its center in such a way as to effect a complete revolution n twenty-four hours. The velocity of rotation is not uniform, and thus the angular displacement of the field varies with the time in accordance with a law which it is important to determine.

Let us represent the celestial sphere by a sphere of unit radius, and each line of sight on the sky by the projection on

this sphere of a straight line passing through the center parallel to this direction.

Let *NESW* be the circle of the true or apparent horizon (Fig. 1); *P* the celestial pole; *Z* the zenith; *PZS* the meridian of the place; *PD* the hour circle of the star *D*, and *D'* the point of the true or apparent horizon toward which the reflected beam is constantly directed.

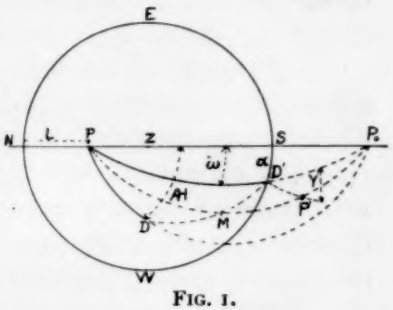

FIG. 1.

The position of the star *D* is defined at any instant by its polar distance $\delta = PD$, and its hour angle $AH = SPD$ counted positively in the direction of the diurnal motion, from the east *E* toward the west *W*. Similarly the point *D'* is determined by its polar distance $\rho = PD'$ and by the angle $\omega = SPD'$, which the plane *PD'* makes with the meridian. We will call this plane *SPD*, which is the hour circle passing through the point *D'* extended, the *reference plane*.[1]

If in place of ρ and ω there were given the azimuth $a = SD'$ and the arc *PS*, the supplement of the latitude *L*, ρ and ω would be calculated by the aid of the two following expressions furnished by the right triangle *PSD'*:

$$\cos \rho = \cos a \cos L, \quad \tan \omega = \frac{\tan a}{\sin L}.$$

In order that the beam coming from the star *D* shall be constantly reflected to *D'*, it is necessary and sufficient, according to the laws of reflection, that the projection *M* of the normal to the mirror shall be maintained by the mechanism at the middle of the arc of the great circle *DD'*. Knowing at any instant the

[1] The use of stereographic projection on the circle of the horizon permits all of these arcs of circles to be rigorously traced out: it is well to adopt it in order to verify graphically the size and direction of the calculated angles. But this manner of projection has the inconvenience of so greatly distorting the sides of the spherical triangles which rise from the circle of the horizon that the use of these projections is more troublesome than useful for the clearness of the demonstrations. For this reason schematic figures are employed here instead of any regular system of projection.

projection M of this normal, we can draw the projection of the direction in which a ray emitted from any point whatever of the celestial sphere is reflected by the mirror: it is only necessary to join this point to the point M by an arc of a great circle and to prolong this arc through an equal distance. Thus the image P' of the pole P is on the arc PM prolonged to the point P', so that $MP' = MP$. The reflected spherical image of the various points of the celestial sphere is thus at any instant symmetrical to their direct position with respect to the point M.

It follows from this that the orientation of the field of view is wholly determined by the knowledge of the reflected image of any given point except that occupied by the star at the center. The pole P, because of its fixed position on the celestial sphere, is particularly adapted for this purpose, and its image P' constitutes a most simple and convenient standard of comparison.

We will now calculate for each instrument the distance and the orientation of the image P' of the pole, *i. e.*, the length of the arc $D'P'$ and the angle Y made by this arc with the great circle $PD'P_0$, the projection of the *reference plane*.

Siderostat.—This name is used to designate the apparatus specially constructed for the purpose of sending a reflected beam toward the southern horizon.

The advantage of this arrangement, which was devised by Léon Foucault, is to reduce as far as possible the angle of incidence $DM = D'M$ of the rays coming from stars which at their upper transit culminate near the zenith or the equator: the aberrations of the reflected image caused by imperfections of the mirror are thus materially reduced. Fig. 1 represents the course of the beam coming from the star D and sent by the siderostat in a horizontal direction; it makes with the southern meridian an angle a, which is counted positively toward the west; a is generally a small fraction of a right angle.

1. *Distance $D'P'$ from the image P' of the pole to D', the center of the field.* The arc $D'P'$ is the side of the triangle $MD'P'$ symmetrical with the triangle MDP, since $MD' = MD$

and $MP' = MP$. These two triangles are equal, as they have an equal angle at M included between two equal sides. The two sides $D'P'$ and DP opposite the equal angle are thus equal: $D'P' = DP = \delta$. Thus *the distance $D'P'$ from the image of the pole to the image of the star (center of the field) is equal to the polar distance of the star observed.* From this it follows that *the image of the pole describes about the center of the field a circle with a radius equal to the polar distance of the star observed.*

2. *Orientation of the arc $D'P'$.*—Let Y be the angle which the arc $D'P'$ makes with $D'P_o$, the prolongation of the projection of the reference plane DP. $Y = P_oD'P' = \pi - PD'P' = \pi - (PD'D + DD'P') = \pi - (PD'D + PDD')$, for $DD'P' = PDD'$ on account of the equality of the triangles MDP and $MD'P'$. The desired angle Y is thus the supplement of the angles at the base of the triangle PDD', the apex of which is at P. From Neper's formula

$$\tan \tfrac{1}{2}(B + C) = \frac{\cos \frac{1}{2}(b - c)}{\cos \frac{1}{2}(b + c)} \cot \frac{A}{2},$$

we have, by substituting $A = DPD' = AH - \omega$, $b = \rho$, $c = \delta$,

$$\tan \tfrac{1}{2} Y = \frac{\cos \frac{1}{2}(\rho - \delta)}{\cos \frac{1}{2}(\rho + \delta)} \tan \tfrac{1}{2}(AH - \omega),$$

an expression which gives the orientation of the arc $D'P'$ and hence the law of rotation of the field of view, as AH varies proportionally to the time.

If we take as the origin of time the moment when the observed star is in the reference plane, $t = 0$ for $AH - \omega = 0$, and for the unit of time the sidereal or solar day (depending upon the object observed), we have $AH\omega = 2\pi t$ and the expression for Y takes the symmetrical form

$$\tan \tfrac{1}{2} Y = K \tan \tfrac{1}{2} 2\pi t,$$

where

$$K = \frac{\cos\frac{1}{2}(\rho - \delta)}{\cos\frac{1}{2}(\rho + \delta)} \quad \text{and} \quad AH - \omega = 2\pi t.$$

It immediately follows :

a. That the rotation of the field has the same period as the diurnal motion.

b. It is continuous and always in the same direction, direct or inverse, according to the sign of K.

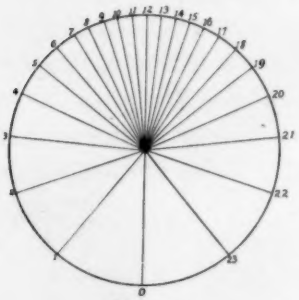

FIG. 2.

c. The reference plane is a plane of symmetry, for the angle Y takes equal values with contrary signs for equidistant epochs on opposite sides of the origin of time.

This law of rotation might be represented geometrically by a curve plotted with the time as abscissa and the angle Y as ordinate. But a more direct configuration of the rotation of the field may be obtained by considering the arc $D'P'$ as the moving radius vector of the circle described by P', the image of the pole, and by tracing the successive divisions of this radius vector at equidistant epochs, aliquot parts of a day. Fig. 2 is a representation of this character on the plane tangent to the sphere at D': the twenty-four successive positions of $D'P'$ are projected as straight lines ; they correspond to a subdivision of the day into twenty-four hours. The origin of time $t = 0$ corresponds to $D'P_0$, the projection of the reference plane and axis of symmetry.

3. *Expression for the angular velocity.*—The angular velocity of rotation at the epoch t is obtained by taking the derivative of the expression for Y with respect to t; after making the necessary reductions we obtain the formula

$$\frac{dY}{dt} = 2\pi \frac{K}{\cos^2 \pi t + K^2 \sin^2 \pi t}.$$

The denominator is necessarily positive, the velocity always

having the sign of K; it varies periodically between the value $2\pi K$, corresponding to the epochs $t = 0, 1, 2, \ldots$ and the value $\frac{2\pi}{K}$, corresponding to the intermediate epochs $t = \frac{1}{2}, \frac{3}{2}, \frac{5}{2}, \ldots$ passing through the value 2π, the angular velocity of the diurnal motion, at the epochs given by the condition

$$\cos^2 \pi t + K^2 \sin^2 \pi t = K$$

or

$$\tan \pi t = \frac{\pm 1}{\sqrt{1 + K}}.$$

As the positions of the star which are most favorable for observation (upper transit) are near the reference plane $t = 0$, the velocity of rotation may be considered as constant and equal to $2\pi K$, for this velocity varies but little in the neigborhood of $t = 0$, since it corresponds to a maximum or a minimum. The velocity $\frac{2\pi}{K}$ is never realized with the siderostat, with which lower transits cannot be observed.

The unit of angular velocity is evidently 2π or one circumference per day; if another unit is preferred, for example, to express the velocity in minutes of arc per minute of time or more generally in n^{ths} of a circumference per m^{ths} of a day, it is only necessary to substitute $\frac{n}{m}$ for 2π. This change of units amounts to the same thing as placing

$$\frac{Y}{2\pi} = \frac{y'}{n}, \qquad \frac{t'}{1} = \frac{\pi}{m},$$

whence

$$\frac{dy'}{d\pi} = \frac{1}{2\pi} \frac{n}{m} \frac{dY}{dt}.$$

As there are $n = 360 \times 60$ minutes of arc in the circumference and $m = 24 \times 60$ minutes of time, the velocity $2\pi K$, or $\frac{n}{m} K$, is here equal to $15 K$; we thus find $15'$ of angle per minute of time for the angular velocity of the diurnal motion $K = 1$.

4. *Direction of rotation of the field of view.*— Let us suppose that the observer is receiving the luminous beam; he thus looks

toward the center of the sphere along the radius which terminates at D', whence it results that the direction of the motion of rotation will be that which an observer placed in the direction D' outside of the sphere will attribute to the motion of the arc $D'P'$. From the expression for Y it is evident that Y and $RA = \omega$ will be of the same sign if the coefficient K is positive. The direction of the diurnal motion, *i. e.*, the direction of the positive variation of RA, is known; it is seen in the figure that when the right ascension of the star D increases the arc PD, seen from outside the sphere, turns in the direction of the hands of a watch; thus for a positive value of K, Y varies in the same manner. The condition under which K is positive is evidently

$$\cos\tfrac{1}{2}(\rho+\delta) > 0, \quad \tfrac{1}{2}(\rho+\delta) < \frac{\pi}{2}, \quad \delta < \pi - \rho.$$

Whence we conclude

When the polar distance of the observed star is less than the supplement of the polar distance of the reflected direction, the apparent direction of rotation of the field of view of the siderostat is that of the hands of a watch.

It is in the contrary direction if the polar distance of the star is less than this supplement. Observation with an astronomical telescope does not change the direction of rotation: the *reversal* of the image is confined to turning through 180° the direction of the origin $D'P_0$.

5. *Critical polar distance: fixed field of view.*—The point of transition between these two cases corresponds to the condition $K = 0$, that is $\cos\frac{1}{2}(\rho+\delta) = 0$; the value of Y remains constantly zero, whatever be the right ascension of the star. Hence

The field of view of the siderostat remains rigorously fixed when the polar distance of the observed star is equal to the supplement of the polar distance of the direction of reflection.

This case of absolute immobility of the field has a corresponding geometrical peculiarity which renders the result evident. It is in fact easily shown that if $\rho + \delta = \pi$, the arc $PM = \frac{\pi}{2}$; the normal to the mirror becomes normal to the line joining the

poles, and thus the mirror is parallel to the Earth's axis. Furthermore the arc PM bisects the angle DPD', and consequently the mirror turns through an angle equal to half the change in the hour angle. These are the two characteristic conditions of the *Coelostat* of M. Lippmann, a very simple instrument which gives an absolutely fixed image of the sky. It consists of a mirror turning about an axis parallel to its own plane and to the Earth's axis with an angular velocity equal to half that of the diurnal motion and in the same direction.

The siderostat may thus replace the coelostat for a region of the sky surrounding a star of polar distance δ. It is only necessary to send the reflected beam in a direction such as to satisfy the condition $\rho + \delta = \pi$, *i. e.*, along one of the generators of the cone of revolution which makes with the Earth's axis the angle $\pi - \delta$, the supplement of the polar distance. It is well to be acquainted with this property of the siderostat, for in certain cases it can be used without serious inconvenience.

6. *Siderostat oriented in the meridian.*— This is the most common arrangement of the siderostat: the horizontally reflected beam is directed exactly toward the south. It follows from this that $\omega = 0$, $\rho = \pi - L$, L being the latitude. The angle Y is the angle which the arc $D'P'$ makes with the meridian, which now becomes the plane of reference and of symmetry. The expression for Y takes the form

$$\tan \tfrac{1}{2} Y = K \tan \tfrac{1}{2} AH,$$

where

$$K = \frac{\sin \frac{1}{2}(L - \delta)}{\sin \frac{1}{2}(L + \delta)}.$$

The above propositions then become very simple.

With the siderostat oriented in the meridian, the field of view is fixed when the polar distance of the observed star is equal to the latitude of the place of observation. The rotation of the field is in the direction of the hands of a watch if this polar distance is less than the latitude; in the inverse direction if it is greater.

The coefficient K, which defines the practically constant value of the velocity of rotation of the field as a function of the diurnal rotation, is always less than unity.

The following numerical values are for the latitude of Paris $L = 48° 50'$, δ increasing by successive steps of 10° (upper transit).

δ	K	δ	K
0° (pole)	1.000	70°	−0.213
10	0.677	80	−0.298
20	0.440	90	−0.376
30	0.258	100	−0.448
40	0.105	110	−0.518
48 50 L	0.000	120	−0.585
50	−0.013	130	−0.651
60	−0.120		

Heliostat.—This instrument sends the reflected beam in the direction of the northern horizon, rarely beyond N. E. or N. W.

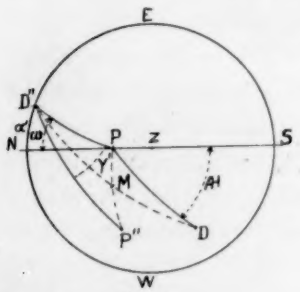

FIG. 3.

Fig. 3 represents the beam coming from the star D and sent in the horizontal direction D'', making with the north meridian an angle $NPD'' = a'$, counted positively toward the east. The pole is at P, SPD is the hour angle and δ the polar distance of the observed star D. We shall designate by $\omega' = NPD''$ and $\rho' = PD''$ the hour angle and the polar distance of the direction of reflection D'', which may be deduced as before from the azimuth a' and the latitude L by means of the right triangle NPD''. The normal M to the mirror is at the middle of the arc DD'', and the image P'' of the pole is on the arc PM produced so that $MP'' = MP$.

The angle of rotation of the field will be determined by the angle made by the arc $D''P''$ with the projection of the reference plane PD''.

Let $Y' = PD''P''$ be this angle; it might easily be deduced from the expression for Y for the siderostat, which is similarly defined. But it is simpler to calculate it directly.

The triangles PDM and $P''D'M$ are equal, as they have an equal angle at M included between two equal sides $DM=D'M$, $PM=P''M$. Thus $D''P''=PD=\delta$.

Thus, as in the case of the siderostat, *the image of the pole reflected by the heliostat describes about the center of the field a circle with a radius equal to the polar distance of the observed star.*

Furthermore,

$$Y'=PD''P=PD''D+DD''P''=PD''D+D''DP',$$

in consequence of the equality of the two triangles PMD and $P''MD''$. The angle Y' is thus the sum of the two angles at the base of the triangle PDD'', whose apex is at P. From Neper's formula referred to above we obtain, after substituting

$$b=\rho'',\ c=\delta,\ \text{and}\ A=\pi-AH+\omega',$$

$$\tan\tfrac{1}{2}Y'=\frac{\cos\frac{1}{2}(\rho'-\delta)}{\cos\frac{1}{2}(\rho'+\delta')}\tan\tfrac{1}{2}(AH-\omega').$$

Y is counted positively in the direction of the hands of a watch. This expression may also be put in the form

$$\tan\tfrac{1}{2}Y'=K'\tan\tfrac{1}{2}2\pi t,$$

where

$$K'=\frac{\cos\frac{1}{2}(\rho'-\delta)}{\cos\frac{1}{2}(\rho'+\delta)}\quad\text{and}\quad AH-\omega'=2\pi t.$$

Thus we again obtain the three conclusions (a), (b), (c), demonstrated above for the siderostat. It is unnecessary to repeat the discussion, which would be quite similar; but we wish to call special attention to the practical points of difference between the two instruments. With the heliostat the coefficient K' is always greater than unity and retains the positive sign under the conditions in which the heliostat is ordinarily employed, *i. e.*, in the observation of the upper transit of heavenly bodies near the zenith or the equator, reflected in a

direction which does not greatly differ from that of the northern horizon.

By developing the value of the cosine we may write K' in the form

$$K' = \frac{1 + \tan\frac{1}{2}\rho' \tan\frac{1}{2}\delta}{1 - \tan\frac{1}{2}\rho' \tan\frac{1}{2}\delta}.$$

The + sign of the coefficient K' here corresponds, as may be seen in the figure, to a variation in Y' contrary in direction to that of the hour angle AH. Hence we conclude that

The field of view of the heliostat, under the actual conditions of observation, turns with an angular velocity which is always greater than that of the diurnal motion; the direction of rotation is that of the hands of a watch.

This conclusion puts in evidence a new cause for the inferiority of the heliostat as compared with the siderostat. To the inconvenience arising from the reflection at great angles of incidence from the mirror of the heliostat must be added that of a great velocity of rotation of the field of view. These two conditions are unfavorable for observations which require in the images both great perfection and complete stability. On account of this fact the siderostat is to be preferred for astronomy of precision.

But this rapidity of rotation of the field is not always an inconvenience; for certain astrophysical observations it is on the contrary advantageous, in that it dispenses with the necessity of employing complex and delicate optical arrangements; here is an example.

Suppose we project, with the aid of a suitable objective, the solar image reflected by a heliostat on to the slit of a spectroscope of high dispersion for the purpose of studying the displacement of lines due to the motions of the solar surface. The most favorable condition occurs when the solar equator is normal to the slit; if the image is oscillated in such a way as to make the opposite limbs of the disk successively tangential to the slit, one may obtain twice the maximum displacement due to

the difference of the radial velocities at the equator (method of oscillating lines).

Except under unusual circumstances the image of the solar disk will not be found in this favorable azimuth and will have little chance of attaining it if a siderostat is employed, since with this apparatus the velocity of rotation of the field of view is zero or very small.

In order to bring the equator to the required azimuth it is necessary to make use of an auxiliary apparatus consisting, for example, of an isosceles total reflection prism, movable about an axis parallel to its base; rotation of this prism changes the azimuth of the Sun's disk by twice the angle, which permits the equator to be placed perpendicular to the slit in the two successive positions 180° apart, which give the maximum double displacements in the inverse order. But the prism must be very perfect both as to material and flatness of the surfaces. Moreover, the rotating mounting which carries it is rather difficult to construct and adjust.

With the heliostat, the natural rotation of the field of view renders this auxiliary apparatus unnecessary; it is sufficient to await the effect of this rotation and to allow the solar equator to place itself perpendicular to the slit. At certain times of the year and for certain orientations of the slit and of the beam reflected by the heliostat, this condition of perpendicularity occurs twice in the same day within an interval of a few hours, the image of the solar equator turning through 180°.

This result, which I discovered experimentally and observed on several occasions, greatly surprised me at first; I had supposed that about twelve hours would be required for the reflected image of the solar disk to turn 180° about its center. The search for an explanation of this phenomenon led to the preparation of this paper. The complete discussion would require rather extended developments. I shall confine myself here to indicating the principle of the demonstrations.

The explanation is based upon the relative velocity of the field of the heliostat when the observed star is near the equator

($\delta = 90°$). The coefficient K', which measures it as a function of the diurnal rotation, varies between 2 and 5 for positions of the Sun between the two solstices.

The accompanying table gives the values of K' in the usual case where the heliostat is oriented in the meridian, the reflected beam being directed horizontally toward the north; we substitute in the formula $\omega' = 0$, $\rho' = L = 48° 50'$, δ increasing by steps of 10° (upper transits).

δ	K'	δ	K'
0° (pole)	1.000	70°	1.932
10	1.083	80	2.231
20	1.174	90 (equinox)	2.663
30	1.277	100	3.358
40	1.396	113 37 (winter solstice)	5.489
50	1.537	110	4.687
60	1.710	120	8.359
66 38 (summer solstice)	1.849		

The value of K' approaches infinity, which it attains when the star is at the southern horizon; this is in fact a critical polar distance $\rho' + \delta = \pi$, which corresponds to grazing incidence at the mirror.

If we know the value of K' we can calculate the time which elapses between the epochs t_1 and t_2, between which the image of the field has turned through 180°. Let Y_1' be the value of the angle Y' at the epoch t_1, when the solar equator, for example, is normal to the slit of the spectroscope, and $Y_2' = Y_1' + \pi$, the value of Y_1' increased by 180° at the epoch t_2. We shall then have the two conditions

$$\tan \frac{1}{2} Y_1' = - K' \tan \pi t_1 \quad \cot \frac{1}{2} Y_2' = K' \tan \pi t_2.$$

Multiplying member by member we finally obtain

$$\tan \pi t_1 \tan \pi t_2 = - \frac{1}{K'^2}.$$

The minus sign shows that the two epochs t_1 and t_2 (supposed to be as near together as possible) are of contrary sign, which signifies that the two corresponding positions of the star are situated on opposite sides of the reference plane (in this

case the plane of the meridian); it is necessary to except the limiting cases for which $t=0$ and $t=\frac{1}{2}$. Let $\theta=t_2-t_1$ be the difference between the two epochs; if t_1 is given t_2 may be calculated. It is especially interesting to find the two epochs for which this difference is a minimum.

Let us therefore make $d\theta=0$, that is, $dt_2-dt_1=0$, and let us differentiate the expression which connects t_1 and t_2; we obtain, after completing the operation,

$$\sin \pi (t_2+t_1)\cos \pi (t_2-t_1)=0.$$

This is the solution $t_2+t_1=0$ which gives the desired minimum; the other, $t_2-t_1=\frac{1}{2}$, gives the 12-hour maximum, which is of no interest.

The two desired epochs symmetrical with respect to $t=0$ are of equal length and contrary sign; substituting, in order to obtain their absolute values, we have

$$\tan \pi t=\frac{1}{K'}.$$

If we give K' increasing values starting from $K'=1$ (uniform rotation), which gives $t=\frac{1}{4}$ of a day, or 6 hours, and $t_2-t_1=12$ hours, we find that the interval $\theta=t_2-t_1$ grows smaller and smaller. Making the calculation to determine this difference θ for the three most interesting epochs relating to the Sun we obtain

	θ
Summer solstice	$7^h\ 54^m$
Equinox	5 30
Winter solstice	2 45

results which demonstrate the possibility of seeing the solar equator turn through 180° in much less than 12 hours.

It is, however, unnecessary that the rotation should be exactly 180° in order to show successively the two inverse effects of the oscillating lines, for the absolute velocity of the solar parallels diminishes only $\frac{1}{10}$ up to ± 25° heliocentric latitude, so that a displacement of 180°—50°=130° is sufficient to show the double phenomenon in the clearest manner.

It remains to determine the orientations of the reflected beam which are most favorable for observation; but this problem is rather complex and deserves to be treated separately.

What precedes is sufficient to show that even in those peculiarities of instruments which at first sight appear to be unfortunate imperfections, possibilities exist which may be turned to account in other classes of work. The complete study of the geometrical properties of instruments commonly reveals some peculiarity capable of rendering unexpected services.

PARIS,
January 1900.

Minor Contributions and Notes

ON THE PROBABLE ORIGIN OF SOME OF THE LINES OBSERVED IN THE SPECTRA OF STARS AND OF THE CHROMOSPHERE.

Sir J. N. Lockyer, in his "Photographic Spectra of Some of the Brighter Stars" (*Phil. Trans.*, **184**, 677, 1893), gives a line λ 4172 ,as one which occurs in *γ Cygni*, in *Bellatrix γ Orionis*, and also in *α Orionis*. In the spectrum of this latter star (*loc. cit.*, Plate 28), 4172 is represented as being an iron line observed in the flame spectrum of iron.

The oxhydrogen[1] flame spectrum of iron has been photographed as it is emitted from very pure metallic iron and by a large number of compounds, including such as yield metallic iron in the highest degree of purity, and with various degrees of dispersion, inclusive of that obtained with a Rowland grating of 21.5 feet radius, belonging to the Royal University of Ireland, but in no instance have photographs been obtained showing an iron line at or sufficiently near to λ 4172 to account for the existence of a line to which this wave-length could be attributed.

As, however, the flame spectra usually observed in the working of the basic Bessemer process, or obtained from meteorites, samples of commercial iron, about seventy iron ores from various localities and of different kinds, and from various rocks and minerals, have been found to show the *gallium line* with a wave-length according to recent measurements of 4172.214 (Hartley and Ramage), it appears most probable, considering the volatility of the metal, that the line observed was that of gallium, and that it is to this the wave-length 4172 has been assigned. The recognition of gallium in terrestrial matter and in the solar spectrum has been confirmed by Mr. Lewis E. Jewell (this Journal, **9**, 229, 1899) by an examination of Professor Rowland's photographs of aluminium, lead, silver, the solar spectrum, and the spectra

[1] Hartley, "Flame Spectra at High Temperatures," *Phil. Trans.*, **185**, 161, 1894; this Journal, **9**, 214 and 221, Hartley and Ramage.

of meteorites, and the wave-lengths of the gallium lines have been determined to be 4172.211 and 4033.224 *Fe-Mn* (Rowland). The ends of the gallium lines on the photographs of the solar spectrum, taken by Mr. Ramage and myself, were observed to be quite distinct from these, but in the center, where the lines are thicker and of greater intensity, this fact was not noticed, though a shorter exposure or a narrower slit would have made it evident. The relative intensities of the gallium lines were the same on all the photographs, whether taken from the oxhydrogen flame, the arc (bright and reversed), or spark spectra; and they are fairly represented by 1 and 00 on Rowland's scale. In the spectrum of α *Orionis* there is a group of four lines attributed to manganese, lying between 4030 and 4034. There is a manganese line given by Rowland as occurring in the solar spectrum at wave-length 4033.224, attributed both to iron and manganese; the second or weaker line of gallium lies at 4033.112. The latter is so frequently present in iron and iron ores, along with the stronger and less refrangible line, that unless its absence is proved its presence must be suspected. But the fact is to be particularly noted that in cases where the manganese was strong this gallium line was obscured by one of the broader lines of manganese, probably that indicated above. Finally, attention may be drawn to the fact that the line 4172 has been recorded as occurring in the following stars:

α *Cygni*	Intensity 4, where the maximum is 6.
β *Orionis*	Intensity 1, where the maximum is 6.
β *Tauri*	
67 *Ophiuchi*	
δ *Cygni*	
γ *Orionis*	
β *Canis majoris*	
γ *Cygni*	
α *Orionis*	

It appears therefore very probable that in α *Orionis* the line 4172 and one of those lying between 4030 and 4040 are gallium lines; and that the less refrangible of the gallium lines also occurs in the other eight stars mentioned above. The *Phil. Trans.*, **187**, 606, 1896, contains an account of the comparison of the spectrum of the chromosphere with the Fraunhofer lines between wave-length 410 and 430, being a preliminary investigation of the results obtained by observations of the total eclipse of the Sun of April 16, 1893. Sir J. N.

Lockyer there gives the wave-lengths of the two lines in the chromosphere and prominences as 4172.2 and 4033.22 (see pp. 612 and 613), with their intensities at different heights; the maximum number representing the intensity is 10:

λ 4172.2						
Photographs	(7)	(8)	(21)	(22)		
Miles	1660	2000	1650	Base		
Intensity	1	3	1	5		
λ 4033.22						
Photographs	(7)	(8)	(9)	(10)	(21)	(22)
Miles	1660	2000	3000 to 3800	5480 to 9600	1650	Base
Intensity	1	1	1	1	3	3

Another line is given, λ 4171.9, taken from the African photograph No. 20. Its intensity in the chromosphere arc is 1, at the base of the chromsphere, 3. This is probably identical in its origin with λ 4172.2, and it appears in the highest degree probable that 4172.2 and 4033.22 are the lines of gallium corresponding to the solar lines 4172.211 and 4033.112 in Rowland's revised table of wave-lengths in this region (this JOURNAL, p. 229, 1899).

This metal being very easily volatilized, it is quite conceivable how its vapor may be separated from that of iron by a process resembling fractional distillation, the more volatile metal being carried to a greater elevation.

W. N. HARTLEY.

MME. CERASKI'S SECOND *ALGOL* VARIABLE.[1]

ANOTHER remarkable variable star of the *Algol* class has been discovered by Mme. Ceraski, and is announced in the *A. N.*, **151**, 223. The position for 1900 is R. A. $= 19^h\ 42^m.7$, Dec. $= +\ 32^\circ\ 28'$. From an examination of the Draper Memorial photographs of this star, it appears that while the star has its full brightness on 45 of them, on several of the early photographs it is so faint that they must have been taken when the star was near minimum. The Moscow photographs furnish the means of determining the period from an interval of four years, the Harvard photographs increase this interval to nine years.

[1] *Harvard College Observatory Circular* No. 47.

The following table gives, in the first seven lines, the results derived from the Harvard photographs; the next four, the results of the Moscow photographs; and the last line gives the estimate of M. Blajko. The times of minima as found by Professor Ceraski may be expressed by the formula *J. D.* 2,415,004.971 + $6^d.0065$ *E*. Measures of four Harvard photographs when the star had its full brightness gave the photographic magnitudes, 11.00, 10.80, 10.74, and 10.79; mean 10.83. The value of *E*, derived from the above formula; the year, month, and day; the Greenwich mean time of the middle of the exposure; the corresponding time expressed in Julian days and decimals; and the duration of the exposure in minutes are given in the first five columns of the table. The sixth column gives the photographic magnitude, and the seventh, the phase computed by means of the formula mentioned above. The error in the ephemeris is given in the eighth column, and is derived from an approximate light curve. It appears that the period is too long by about $0^m.6$, and if this correction is applied, the errors have the values given in the ninth column. The tenth column gives the mean photographic magnitude, during the entire time of exposure, derived from the corrected ephemeris and light curve.

E	Date	G. M. T.	J. D.	Ex.	Mag.	Phase	O–C	O–C¹	C. M.
	y m d	h m		m					
−580	1890 6 2	16 32	1521.689	27	11.81	+.488	+.25	+.01	11.7
−570	1890 8 1	14 20	1581.597	13	12.75	+.331	+.24	+.02	12.7
−570	1890 8 1	14 57	1581.623	20	12.22	+.357	+.16	−.06	12.5
−557	1890 10 8	11 53	1659.495	10	<12.4	+.145	.....	.....	12.8
−508	1891 8 8	16 07	1953.672	20	<11.7	+.003	.....	.....	12.1
−443	1892 9 2	14 44	2344.614	16	10.96	+.522	+.18	.00	11.0
−325	1894 8 11	15 07	3052.630	10	10.90	−.228	+.13	−.01	10.9
−257	1895 9 24	10 11	3461.424	102	<12	+.124	.....	.....	12.9
−254	1895 10 12	6 45	3479.281	300	<12	−.039	.....	.....	12.5
− 97	1898 5 12	10 12	4422.425	105	<12	+.085	.....	.....	12.9
− 84	1898 7 29	10 22	4500.432	125	<12	+.008	.....	.....	12.9
− 0	1899 12 16	3 33	5005.106	125	Ft.	+.135	.....	.....	12.5

It appears from this table that while the formula of Professor Ceraski satisfies all the later observations, it is not confirmed by the early observations. For instance, according to this formula the star should have had nearly its full brightness on the first three photographs. On the other hand, all the observations are satisfied by the corrected formula, in which the period is $6^d\ 0^h\ 8^m.8$ As soon as we

obtain accurate observations of subsequent minima, these combined with the photographs taken in 1890, will give a much more precise formula. A comparison of the sixth and tenth columns shows that the observed and computed magnitudes differ in one case only by more than one tenth. A slight defect partially covers the image of the variable on the second plate taken August 1, 1890, and thus renders the measured value too bright. The period differs so little from exactly six days that for a long time the minima cannot be observed in certain longitudes. Accordingly, while valuable observations may be obtained next autumn in Europe, or better still in Asia, minima cannot be observed in America until the following year.

Five stars of the *Algol* class, S *Cancri*, U *Cephei*, W *Delphini*, $+45^{\circ}3062$, and the star here discussed, are especially interesting, owing to the large variation in their light, which amounts to about two magnitudes in each case. It is remarkable that two of these were found by Mme. Ceraski, and one by her distinguished husband.

EDWARD C. PICKERING.

February 12, 1900.

ON THE PROBABLE ERROR OF A RADIAL VELOCITY-DETERMINATION.

THE practice of following the radial velocity of a star by what purports to be its probable error, computed from the measures of only one plate, seems to me to be incorrect; and, in the absence of special interpretation, misleading. Such a determination of probable error is based on only a small part of the observation. It assumes that the plate is perfect: containing no injurious effects of temperature variation, flexure, jarring, imperfect adjustments, etc. Save for peculiarities in the distribution of the silver grains, it is affected by little beyond the errors in the assumed intervals between the lines, and by accidental errors in the micrometer bisections of the lines.

A meridian circle observer, in determining the right ascension of a star, notes its times of transit over a number of lines in the reticle. From these and the assumed intervals between the lines, he can compute the probable error of the mean of the observed times of transit. If x is this probable error, it would be incorrect to say that the observed right ascension of the star is

$$11^{h}\,12^{m}\,13^{s}.14 \pm x,$$

since this would disregard many important factors in the complete observation. Yet such a practice would be fairly analogous to that discussed above for the spectrograph.

It seems to me that the probable error of a single velocity-determination should be derived from the results of several complete observations, unless it has been shown by long experience that the probable error yielded by the measures of one plate is in fair agreement with the value obtained from a comparison of the results from several plates. The substantial agreement of the values obtained by the two methods would indicate: first, that the plates were free from the accidental effects of flexure, temperature changes, etc.; and second, that the observer's habit of measurement remained constant. The first of these desiderata has, I believe, been practically attained, for the best stars, with the Mills spectrograph. The second is, from its nature, more difficult to control, and necessitates frequent checks.

W. W. CAMPBELL.

LICK OBSERVATORY,
January 15, 1900.

CATHERINE WOLFE BRUCE.

ASTRONOMERS the world over will learn with deep regret of the death of Miss Catherine Wolfe Bruce, news of which reaches us just as we go to press. For years confined to her room by an ever increasing illness, and personally known by but few of the many who have benefited by her bounty, Miss Bruce has nevertheless endeared herself to men of science at home and abroad, aiding as perhaps no other has done the progress of research. Recognizing no national boundaries, giving assistance where it was most needed, and seeking no fame for herself, Miss Bruce may well be regarded as one of the most sympathetic and generous patrons astronomy has ever known. Many a project, which without her assistance would have come to naught, has been successfully developed through her aid. Many an advance in our knowledge of the heavens is due directly to the help she gave. The appeals that came to her from far and wide were received with the kindliest consideration, both by herself and also by her sister, Miss M. W. Bruce, who often acted in her stead. Astronomers in almost every country of the civilized world know from their own experience how prompt was

the response, and how often it took the affirmative form. In common with many others who have received less direct advantage from her gifts to science, they will sincerely mourn her loss.

ERRATA.

Vol. VIII, p. 221, in the formula, *for* 404.251 *read* 464.251.

Vol. X, p. 189, in the formula, for $R_0 - R$ read $R - R_0$.

Vol. X, p. 277, *for* 0.0043053 *read* 0.0043083; *for* 4325.57 *read* 4235.27. In formula (1) *for* $[\lambda_3]$ *read* $[\lambda_3 - \lambda_0]$.

Vol. X, p. 278, *for* 4425.52 *read* 4442.52; *for* 4425.36 *read* 4442.36.

Vol. X, p. 350. The entire passage in Professor Hasselberg's article, beginning "It appears, first, that numerous exact coincidences" etc., on p. 350, and ending ". . . . this is also independent of the titanium line," on p. 351, should immediately follow the table ending at the bottom of p. 357.

Reviews

PROFESSOR J. J. THOMSON'S WORK ON THE STRUCTURE OF THE ATOM.

1. *The Discharge of Electricity Through Gases.* Princeton Lectures. J. J. Thomson. (Scribner's, 1898.)

2. *Cathode Rays.* J. J. Thomson. *Phil. Mag.*, **44**, 293–316, 1897.

3. *On the Charge of Electricity carried by the Ions produced by Röntgen Rays.* J. J. Thomson. *Phil. Mag.*, **46**, 528–545, 1898.

4. *On the Masses of the Ions in Gases at Low Pressures.* J. J. Thomson. *Phil. Mag.*, **48**, 547–567, 1899.

Those who listened to the lectures of Professor J. J. Thomson at Princeton in the autumn of 1896 will recall the surprise — not to say astonishment — with which was received his clearly stated experimental evidence for thinking that the carriers of electrification, through gases at ordinary pressures, are aggregations of atoms, large compared with a molecule of the gas, so large, indeed, that they can be filtered out with glass wool.

In striking contrast with this stands the principal result of his investigations during the three years that have elapsed since the Princeton Sesqui-centennial, viz., that in gaseous discharges at low pressures the negative electrification is carried by particles of matter — "corpuscles" he calls them — whose mass is of the order of one thousandth of the mass of a hydrogen atom.

Not for many years — perhaps never — has anyone doubted the divisibility of the chemical atom; but the experimental evidence offered by Thomson is so convincing as compared with the arguments of Prout, or as compared with any *a priori* considerations, that his work becomes of the highest interest to all students of physical science, especially those of chemistry and spectroscopy.

The general method of attack, which may be roughly described as *a quantitative study of the charged ion*, is outlined in his paper on "Cathode Rays." The ratio $\frac{m}{e}$, where m is the mass and e the charge of an ion, is a well-established electrolytic constant, an invariant for

any one conducting solution. Indeed, the constancy of this ratio may be considered as a mathematical expression for one of Faraday's Laws of Electrolysis. For hydrogen this ratio has the value

$$\frac{m}{e} = 0.0001035 = 10^{-4} \text{ approximately}.$$

But within the last few years this ratio has been determined for conducting, or "ionized," gases. For this determination Professor Thomson has employed three independent methods, to which may be added a fourth, due to Lorentz. These are briefly and roughly as follows:

1. The deflection of cathode rays by means of a magnetic field.

Here $\frac{m}{e} = \frac{I^2 Q}{2W}$

where
- I is the intensity of a magnetic field multiplied by a radius of curvature,
- Q is a quantity of electrification measured by an electrometer, and
- W is a quantity of heat energy measured by aid of a thermopile, balances, etc.

2. The deflection of cathode rays by means of an electrostatic field.

Here $\frac{m}{e} = \frac{H^2 l}{F\theta}$

where
- H denotes the strength of a magnetic field,
- F, the intensity of the electrostatic field,
- l, a measurable distance, and
- θ, the deflection produced by the field F.

3. The effect of a magnetic field in diminishing the gaseous conduction between electrodes separated by more than a certain critical distance, an effect discovered by Elster and Geitel.

Here $\frac{m}{e} = \frac{H^2 d^2}{2V}$

where
- H is the intensity of a magnetic field,
- d, the critical distance, determined by experiment, and
- V, the potential difference between the electrodes.

4. H. A. Lorentz has shown (*Phil. Mag.*, **43**, 226, 1897) that from the Zeeman effect we have

$$\frac{m}{e} = \frac{H(\lambda_1 - \lambda)}{4\pi V}$$

where
- H is the intensity of the magnetic field producing the Zeeman effect,
- $\lambda_1 - \lambda$, the change in wave-length produced on any ray by the magnetic field, and
- V, the speed of light.

Very striking is the general agreement among the various values of $\frac{m}{e}$ obtained by these diverse methods. Whether employed by Lenard (*Wied. Ann.*, **64**, 279, 1898), and Kaufmann (*Wied. Ann.*, **61**, 544, 1897; **65**, 432, 1898) on the continent, or used with many variations by Thomson at Cambridge, or made to depend upon the Zeeman effect, all these methods lead to a value approximately 1.0×10^{-7}. While the values of this ratio vary, roughly speaking, to the extent of 25 per cent. on either side the mean, and while these differences will doubtless lead to future investigation and to new and interesting results, we may, for the present, say that they are in practical agreement. And what is quite as remarkable is the fact that the value of $\frac{m}{e}$ appears to be entirely independent of the gas employed, a result in marked contrast to that obtained in the case of liquid electrolytes, where $\frac{m}{e}$ varies from one element to another directly as the atomic weight and inversely as the valency.

Observing that in the case of liquids, this ratio is nearly one thousand times larger than in the case of gases at low pressures, Thomson at once set himself the problem of determining whether this discrepancy arises from the fact that the *mass* of the liquid ion is *greater* than that of the gaseous ion, or from the fact that the *charge* of the liquid ion is *smaller* than that of the gaseous ion, or from both facts. As the sequel shows the first of these alternatives is confirmed by experiment. For Thomson succeeded in measuring in C. G. S. units the charge of electricity carried by a negative ion in gas ionized by Röntgen rays (*Phil. Mag.*, December 1898).

The highly original method and carefully executed experiments which led him to his final results, namely,

$$e = 7.3 \times 10^{-10} \text{ electrostatic units}$$

must be followed in the author's own paper.

For space will here permit us merely to say that, by using the negative ions as centers of condensation for water vapor, he determined the number of ions in a cubic centimeter of the gas, and then measuring what practically amounts to the total charge of a cubic centimeter of the gas, he at once obtains the value of e by division. But let no one imagine that these processes, so scantily described, are simple; on the contrary, they require manipulative skill of a high order, and depend upon important results previously obtained by Rutherford, Wilson, and others.

Knowing e and $\frac{m}{e}$ for a gas, we have at once the value of m for that gas. But we have, as yet, no means of getting m for a liquid electrolyte. The one step necessary was supplied by Townsend (*Proc. Roy. Soc.*, **65**, 192, 1899), who showed, by a study of the diffusion of ions into gases, that the charge on an ion produced by Röntgen rays is the same as that carried by the hydrogen atom in ordinary liquid electrolysis. Since the e's are the same in each case, it follows that the divergence in the values of $\frac{m}{e}$ between hydrogen gas and hydrogen in a liquid electrolyte, say H_2SO_4, is due to the fact that the mass of the gaseous ion is about one thousand times smaller than the mass of the hydrogen atom.

The demise of the atom has been long expected; only the hypothetical atom has been regarded as indivisible. But it is doubtful whether many outside the Cavendish laboratory expected the division to come in this way. The outlook along spectroscopic lines appeared more hopeful, especially in the direction of Michelson's powerful separation of so-called homogeneous radiations. The Echelon spectroscope and analysis make a powerful combination and one which probably has much to tell us concerning that complicated dynamical system which gives us the series of Kayser and Runge and which has so long been miscalled an "atom."

Since the mass of Thomson's "corpuscle" appears to be the same for all gases, it would seem that we are here confronted by a veritable *Urstoff*. But the most disappointing feature about it all is that these corpuscles cannot be collected in quantities large enough for one to study their chemical properties. Nevertheless, the work under review is full of suggestion for the chemist. For instance, the fact that practically all lines in the spectrum of an element exhibit the Zeeman effect leads to

the inference that in each atom there are *many* of these corpuscles. While, from the manner in which Lenard rays are absorbed, it is evident that the mean free path of these ions depends only upon the density of the medium, and not at all upon its chemical composition. In facts such as these Thomson finds his warrant for saying that "atoms of ordinary elements are made up of corpuscles and holes, the holes being predominant" (*Princeton Lectures*, p. 198).

Among all chemical problems, it would not be easy to find one which is more fundamental, more profoundly interesting, or more hopelessly difficult, than the dynamical explanation of the periodic law. Unusual interest, therefore, attaches to the slight hint which Thomson has given as to a possible manner in which atoms may be built up out of corpuscles so as to satisfy the demands of Mendelejeff's table. Briefly, the arrangement of the corpuscles in the atom is that of Mayer's groups of magnets floating in a magnetic field. If, in addition, we imagine that certain chemical properties are functions of the number of magnets (corpuscles) in the center of the group, we have a system for which these chemical properties, at least, are periodic functions of the atomic weight. Such an atom, it is pointed out, is not inconsistent with what we know of spectral series; it furnishes a possible mode of conceiving electric conduction in gases, it offers a most plausible explanation of the law of absorption for Lenard rays, and, finally, it brings comfort to those who still regret that Prout's hypothesis—in its original form—is untenable.

Your reviewer's conscience is not quite at rest until he has warned any chance reader not to infer from the above that the director of the Cavendish laboratory has, for a moment, forgotten to draw a sharp line between the results of experiment and the results of imagination—even of the "scientific imagination." Any such impression is the fault of the reviewer, and should be removed at once by a careful reading of the original. For, doubtless, no one has sought so ably as Professor Thomson, or would welcome more warmly than he, any explanation of these phenomena involving an assumption less violent than that of a corpuscle with a mass one thousand times smaller than that of the hydrogen atom.

HENRY CREW.